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I

TRANSACTIONS

OF THE

ROYAL ACADEMY OF MEDICINE IN IRELAND.

TRANSACTIONS

OF THE

Royal Academy of Medicine

IN IRELAND.

VOL. XXXV.

EDITED BY

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SECTION OF MEDICINE.

President's Address - -	(Kidney disease).
Dr. G. E. Nesbitt - -	Remarks on the Allen treatment of diabetes.
Dr. A. R. Parsons and Miss A. Chance - - -	The treatment of severe diabetes by the Allen method, with special reference to its effect on marked acidosis.
Dr. G. Peacocke - -	Note on a case of diabetes under Allen treatment.
Dr. W. M. Crofton - -	Treatment of rheumatism.
Dr. T. G. Moorhead - -	Fevers amongst the troops in Egypt.
Dr. C. M. O'Brien - -	Psoriasis · its etiology with an epitome of ancient and modern methods of treatment.
Dr. W. M. Crofton - -	Some medical problems of the present and future.
Dr. H. C. Drury - -	Case of hyperpyrexia in early typhoid fever.
Dr. G. E. Nesbitt - -	Clinical notes of case of intrathoracic tumour.
Dr. A. R. Parsons - -	Sudden death in tabes dorsalis (cardiac crisis)
Dr. R. J. Rowlette - -	Note on the limitation of vaccine treatment.
Dr. F. C. Purser - -	Shell shock.
Sir John Moore - - -	Nephritis following severe burns.

SECTION OF SURGERY.

The President's Address ·	(Nephrectomy).
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Mr. W. I. de C. Wheeler -	Some surgical impressions from France.
Mr. D. Kennedy - -	Latent period in acute perforation of stomach and duodenum.
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SECTION OF OBSTETRICS.

Dr. J S. Sheill - - -	Case of genital tuberculosis transmitted from husband to wife.
Dr. B. Solomons - -	Note on fibroids and pregnancy.
Sir William Smyly - -	Continuous sponges.
Dr. E. H. Tweedy - -	Modern operation for pyosalpinx.
Dr. J. S. Sheill - - -	Clinical notes of a case of pneumonia in the puerperium.
Dr. Alfred Smith - -	Some observations on the operative treatment of fixed backward displacement of the uterus.
Dr. B. Solomons - -	Notes on a case of melæna and hæmatemesis neonatorum.
Dr. E. H. Tweedy - -	Compression of the uterine vessels by vaginal plugging.

SECTION OF PATHOLOGY.

Sir John Moore - - -	Pathological findings in two cases of hectic fever.
Drs. W. Boxwell, W. M. Crofton, H. C. Drury, T. G. Moorhead and A. R. Parsons	Discussion on nephritis.
Dr. R. J. Rowlette - -	Pathological Report of the Rotunda Hospital for 1914–1916.
Dr. W. M. Crofton - -	Influence of environment on the morphology of the acne bacillus.

SECTION OF STATE MEDICINE.

Dr. W. G. Smith - -	High explosives, T.N.T.: its toxic action in munition workers; detection in the urine.
Sir John Moore - - -	Burns and scalds as a cause of child disablement and death.

SECTION OF ANATOMY AND PHYSIOLOGY.

Prof. A. F. Dixon - -	Some abnormal thoracic vessels.
Drs. J. N. O'Connor and E. J. Conway - - -	The influence of narcosis on heat production (preliminary communication).
Dr. J. R. D. Holtby - -	A factor of use in estimating cranial height.
Dr. J. N. O'Connor - -	A theory of the body temperature.
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1898 Caraher, More, L.R.C.P. & S.I., Glenville, Drumconrath, Co. Meath.

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1903 McVittie, R. B., M.D., 62 Fitzwilliam-square.

1887 McWeeney, E. J., M.D., F.R.C.P.I., Professor of Pathology, University College, Dublin, 84 St. Stephen's-green, Dublin.

1913 Madill, David G., M.B., M.Ch., T.C.D., Assistant Master, Rotunda Hospital, 26 Lower Baggot-street.

1900 Maguire, Katharine M. N., M.D., 67 Merrion-square, South.

1904 Matson, John A., M.D., Univ. Dub., F.R.C.P.I., Assist. Physician Richmond, Whitworth, and Hardwicke Hospitals, 16 North Great George's-street.

1897 Maunsell, R. Charles B., M.B., B.Ch., B.A.O., Dub., F.R.C.S., Surgeon to Mercer's Hospital, 60 Merrion-square.

1912 Meenan, James N., M B., R.U.I., Prof. of Hygiene, University College Dublin, 66 Stephen's-green.

1902 Meldon, G. Pugin, M.D., F.R.C.S., Surgeon Westmorland Lock Hospital, 67 Lower Baggot-street.

1907 Milroy, T. H., M.D. Edin., F.R.S.E., Dunville Professor of Physiology, Queen's University, Belfast, Melvyne, Malone Park, Belfast.

1901 Mitchell, A. B., F.R.C.S., Surgeon Royal Victoria Hospital, 4 College-square, Belfast.

† Moore, Sir John William, M.D., D.Sc. Oxon., Ex-President R.C.P., Physician Meath Hospital, 40 Fitzwilliam-square, West, Dublin.

1904 Moorhead, T. Gillman, M.D., F.R.C.P., Physician City of Dublin Hospital, 23 Upper Fitzwilliam-street.

1883 Murphy, John, F.R.C.P., Physician Mater Misericordiæ Hospital, 13 Merrion-square, Dublin.

1908 Neill, Thomas, M.B., B.Ch., Dub.; Ex-Assistant Master Coombe Hospital, 8 Fitzwilliam-place.

1910 Nesbitt, George E., M.D. Dubl., F.R.C.P.I., Assistant Physician Richmond Hospital, 71 St. Stephen's-green, S.

1899 O'Brien, C. M., M.D., L.R.C.P. & S., Physician to City Hospital for Diseases of the Skin, 29 Merrion-square.

† O'Carroll, Joseph Francis, M.D., LL.D., P.R.C.P., Physician Richmond, Whitworth, and Hardwicke Hospitals, Professor of Medicine, National University, Ireland, 43 Merrion-square, Dublin.

1914 O'Connor, J. M., M.D. N.U.I., Assistant to Professor of Physiology University College, 26 Charleston-avenue, Rathmines.

1906 O'Farrell, Thomas T., F.R.C.S.I., D.P.H., Pathologist, St. Vincent's Hospital, First Assistant in Pathology, University College, Dublin, N.U.I., 26 Upper Leeson-street.

1910 O'Kelly, W. D., M.D., D.P.H., N.U.I., Assistant to Professor of Pathology U.C.D., 26 Upper Mount-street.

† Ormsby, Sir Lambert Hepenstal, M.D., Ex-President R.C.S., Surgeon Meath Hospital, Surgeon National Children's Hospital, 92 Merrion-square, West, Dublin.

1893 Parsons, Alfred Robert, M.D., Univ. Dub., F.R.C.P., Physician Royal City of Dublin Hospital, 27 Lower Fitzwilliam-street, Dublin.

1895 Peacocke, Geo. J., M.D., F.R.C.P.I., Physician Adelaide Hospital, 2 Fitzwilliam-square.

1887 Pearson, Charles Yelverton, M.D., F.R.C.S. Eng., Professor of Surgery Queen's College, 1 Sidney-place, Cork.

1915 Pollock, J. H., M.B., M.Ch., N.U.I., Assistant Pathologist Richmond Hospital, 34 Upper Leeson-street.

1904 Pringle, Seton, M.B., Univ. Dub., F.R.C.S.I., Surgeon Mercer's and Drumcondra Hospitals, 7 Fitzwilliam-place.

† Purefoy, Richard Dancer, M.D., LL.D., Ex-Pres. R.C.S., Ex-Master Rotunda Hospital, 62 Merrion-square, Dublin.

1902 Purser, Frank C., M.D. Univ. Dub., F.R.C.P.I., Physician, Mercer's Hospital, Professor of Practice of Medicine, Royal College of Surgeons, 20 Lower Baggot-street.

† Redmond, Sir Joseph Michael, M.D., Ex-Pres., R.C.P., Physician to Mater Misericordiæ Hospital, 41 Merrion-square, Dublin.

1905 Rowlette, Robert J., M.D., F.R.C.P., Physician Jervis-street Hospital, Pathologist Rotunda Hospital, 42 Lower Baggot-street, Dublin.

† Scott, John Alfred, M.A., M.D., F.R.C.S., Professor of Physiology Royal College of Surgeons, 36 Lower Baggot-street, Dublin.

1906 Sheill, J. Spencer, F.R.C.P.I., Ex-Asst. Master Coombe Hospital, 31 Upper Pembroke-street.

1886 Smith, Alfred J., M.B., F.R.C.S., Professor of Midwifery, University College, Gynæcologist St. Vincent's Hospital, 30 Merrion square, Dublin.

1901 Smith, Trevor N., F.R.C.S., late Assistant Master Coombe Hospital, Jerpoint, Elton Park-road, Sandycove.

† Smith, Walter George, M.D., Ex-President R.C.P., King's Professor of Materia Medica, School of Physic, and Physician to Sir Patrick Dun's Hospital, 25 Merrion-square, Dublin.

† Smyly, Sir William J., M.D., Ex-President R.C.P., Ex-Master Rotunda Lying-in Hospital, 58 Merrion-square, S., Dublin.

1908 Solomons, Bethel A., M.D., Univ. Dub., F.R.C.P.I., Gynæcologist, Mercer's Hospital, 42 Fitzwilliam-square.

1905 Stevenson, Walter C., M.B., B.Ch., B.A.O., Surgeon Dr. Steevens' Hospital 60 Lower Baggot-street.

1908 Stokes, Henry, M.D., Univ. Dub., F.R.C.S.I., Surgeon Meath Hospital, 32 Upper Pembroke-street.

1907 Stoney, R. Atkinson, M.B., B.Ch., Dub., F.R.C.S.I., Surgeon Royal City of Dublin Hospital, 56 Fitzwilliam-square, N.

† Story, John Benjamin, M.B., V.-P.R.C.S., Surgeon Royal Victoria Eye and Ear Hospital, Professor of Ophthalmic and Aural Surgery R.C.S., 6 Merrion-square, N., Dublin.

1895 Taylor, Edward Henry, M.D., Dub., F.R.C.S., Regius Professor of Surgery Dublin University, Surgeon Sir Patrick Dun's Hospital, 77 Merrion-square, Dublin.

1898 Taylor, William, M.B., Pres. R.C.S., Surgeon Meath Hospital, 47 Fitzwilliam-square.

1897 Thompson, W. H., M.D., F.R.C.P.I., King's Professor of Institutes of Medicine, Trinity College, Dublin.

1894 Thompson, Sir W. J., M.D., Univ. Dub., F.R.C.P.I., Registrar, General for Ireland, 59 Fitzwilliam-square.

1911 Tierney. Gerald, M.B., R.U.I., F.R.C.S.I., Assistant Master National Maternity Hospital, 22 Lower Baggot-street, Dublin.

1893 Tweedy, Ernest Hastings, F.R.C.P., Ex-Master Rotunda Lying-in Hospital, Gynæcologist Steevens' Hospital, 6 Fitzwilliam-place, Dublin.

1900 Tweedy, Herbert, L.R.C.P. & S., Colonial Surgeon.

1901 Watson, Edward J., M.D., F.R.C.P., Anæsthetist and Medical Officer in Charge of the X-ray Department Sir P. Dun's Hospital, Demonstrator in Anatomy T.C.D., 25 Fitzwilliam-place.

1915 Webb, Ella G. A., M.D., R.U.I., 20 Lower Hatch-street.

1904 Wheeler, W. I. de C., M.D., B.Ch., B.A.O. Dub., F.R.C.S., Surgeon Mercer's Hospital, 23 Fitzwilliam-square.

1912 White, Reginald J., F.R.C.S.I., 23 Merrion-square.

1903 Wigham, Joseph T., M.D., Assistant to Professor of Pathology T.C.D., 10 Brighton Vale, Monkstown, Co. Dublin.

1898 Winter, W. A., M.D., Dub., F.R.C.P.I., Physician Steevens' Hospital, 17 Fitzwilliam-place.

1893 Woods, Sir Robert H., M.B., M.Ch. *Hon. Causâ*, Univ. Dub., Ex-P.R.C.S., Hon. Professor of Laryngology and Otology, Throat Surgeon Sir P. Dun's Hospital, 39 Merrion-square, Dublin.

1894 Wynne, Albert E., M.D., Univ. Dub., L.R.C.P. & S. Edin., F.R.C.S., Assistant-Surgeon, Drumcondra Hospital, 27 Westland-row, Dublin.

MEMBERS.

1911 Cahill, Frank Kennedy, F.R.C.S.I., 80 Merrion-square, Dublin.

1916 Conway, Thomas W., F.R.C.S.I., 72 Northumberland-road.

1887 Cope, Geo. Patrick, L.R.C.P., L.R.C.S., Medical Officer No. 3 Dispensary District, South Dublin Union, 36 Harcourt-street.

1912 Crawford, H. de L., M.B., B.Ch., Univ. Dub., F.R.C.S.I., 39 Lower Baggot-street.

1892 Day, J. Marshall, M.B., Univ. Dublin, Resident Medical Officer Cork-street Fever Hospital, Dublin.

† Delahoyde, O'Connell J., F.R.C.S., 47 Rutland-square, Dublin.

1912 Ferrar, B. B., M.D., F.Z.S., Superintendent Royal Zoological Society of Ireland, Phœnix Park.

1897 Fleury, Eleonore Lilian, M.D., R.U.I., Richmond District Asylum, Dublin.

1900 Goff, A. S., L.R.C.P. & S., Belfort House, Dundrum.

1889 Goulding, H. Benson, F.R.C.S., 12 Rathmines-road.

1898 Hatch, Richard, L.R.C.P. & S., 146 Pembroke-road.

1912 Hayden, Patrick Edward, F.R.C.S.I., Surgeon to Jervis-street Hospital, 20 Harcourt-street.

1897 Hughes, Charles, L.R.C.P. & S., 16 Lower Fitzwilliam-street.

1913 Maxwell, Euphan, M.D., M.Ch, T.C.D., 19 Lower Baggot-street.

1915 O'Sullivan, J. J., L.R.C.S.I., L.A.H., 32 The Mall, Waterford.

1912 Thomson, T. Mather, L.R.C.P. & S.I., Assistant Physician Meath Hospital, 37 Fitzwilliam-place.

STUDENT ASSOCIATES.

McDonnell, T. P., 97 Grove-park, Rathmines.

Seale, J. Fayle, Meath Hospital.

Woollcombe, Robert Lloyd, LL.D., 14 Waterloo-road, Dublin.

RULES.

1. The name shall be, "ROYAL ACADEMY OF MEDICINE IN IRELAND." (1887.)

Constitution.

2. The Academy shall consist of Fellows, Honorary Fellows, Members, and Student Associates.

Management.

3. The affairs shall be managed by a Council, consisting of the President, Ex-Presidents (1893), Ex-General Secretaries (1905), the six Presidents of Sections, the General Secretary and Treasurer, the Secretary for Foreign Correspondence, six Secretaries of Sections, and ten Councillors, being two representatives from the Medical, Surgical, Obstetrical, and Pathological Sectional Councils respectively, and one from each of the Sectional Councils of State Medicine and of Anatomy and Physiology (1915).

Meetings.

4. The Meetings shall be General and Ordinary.

Publication of "Transactions."

5. The "Transactions" shall be published by the Council, subject to the provisions hereinafter contained.

Original Fellows and Members.

6. All the Members of the present Societies (Medical, Surgical, Obstetrical, and Pathological) shall be Original Fellows or Members, without entrance fee, on payment of the annual subscription on or before 31st December, 1882.[a]

Fellows.

7. Fellows of the Royal College of Physicians of Ireland, and of the Royal College of Surgeons in Ireland, shall be admitted, without ballot, on payment of the entrance fee and the subscription for the current year. All others, being Registered Medical Practitioners not directly or indirectly engaged in the sale of drugs, shall be proposed by two Fellows, and elected by ballot by the Council.

8. Candidates shall be proposed at one Meeting of the Council, and balloted for at the next—one black bean in four to reject.

8A. That all Rules referring to the admission of Fellows, Members, and Student Associates shall be interpreted as referring to Ladies as well as Gentlemen.

Privileges of Fellows.

9. Fellows only shall be eligible for office in the Academy. They shall have the privilege of attending all Meetings of the Academy, of making Communications, and of voting and speaking at such meetings. They shall also receive a copy of the "Transactions."

10. These privileges shall not be exercised by any Fellow in arrear with his subscription.

[a] Those who have paid a Life Subscription to any of the above Societies will be admitted to the privilege of Fellows on payment of Member's subscription

Honorary Fellows.

11. Honorary Fellows, limited in number to 25, may be nominated by the Council, and elected, on motion at a General Meeting of the Academy by a majority of at least two-thirds of those present and voting.

Members.

12. Any Registered Medical Practitioner may be elected as a Member, the election to be conducted in the same manner as that of Fellows.

Privileges of Members.

13. Members shall have the privilege of attending the Ordinary Meetings of the Academy, of making Communications, and of taking part in debate. They can purchase the "Transactions" at cost price.

Associates.

14. Any Registered Medical Practitioner, temporarily resident in Dublin, may be elected an Associate for the period of one Session. The subscription must be paid before election, and the General Council may elect without notice of motion. Such Associates may attend and speak at the ordinary meetings of the Academy.

Student Associates.

15. Registered Medical Students may be elected Student Associates for the period of one year. The Subscription must be paid before election, and the Council may elect without notice of motion.

Student Associates shall have the privilege of attending the Ordinary Meetings of the Academy.

Annual Subscription.

16. Fellows shall pay £2 2s., and Members £1 1s. Associates and Student Associates shall pay 5s. The Subscription shall become due on the 1st of October in each year, and if the Subscription be not paid on or before the first Meeting in February, the defaulter shall cease to belong to the Academy, unless the delay shall be accounted for to the satisfaction of the Council. No Fellow shall vote at the Annual General Meeting who has not paid his subscription for the year. Registered Medical Practitioners not residing within 15 miles of Dublin are eligible as Fellows of the Academy on payment of the entrance fee, and an annual Subscription of £1 1s. Medical Officers on the active list of the Royal Navy and Army are eligible for election as "Temporary Fellows" of the Academy on payment of an annual Subscription of One Guinea. Such Temporary Fellows shall enjoy all the privileges of Fellows, except that of voting at General Meetings. No Fellow who has not paid his Subscription shall vote for any Candidate for Office at the Annual General Meeting, nor can the name of any Fellow be received by the General Secretary for insertion on the ballot paper as a Candidate for Office unless his Subscription shall have been paid for the current year.

Entrance Fee.

17. After admission of Original Fellows, all Fellows shall pay an entrance fee of £1 1s.

Council.

18. The Council shall meet on the last Friday in the month throughout the Session, or oftener should they see occasion—five to form a quorum,

19. Notice of all Extraordinary Meetings shall be transmitted by the Secretary to every Member of the Council. The President or any five Members of Council may call an Extraordinary Meeting of the Council. The Council shall determine questions by vote, or by division if so demanded, the President having a casting vote only. Any regulation of the Council shall have the force of a law, until submitted to the next General Meeting. The Council shall have the power of filling up any vacancies which may occur in the list of Officers of the Academy, except that of President, before the Annual General Meeting. If a vacancy in the office of President should occur, the General Council shall summon a Special General Meeting of the Academy to fill such vacancy. (1888.)

Sectional Councils.

20. There shall be six Sectional Councils elected by the Annual General Meeting in October, termed respectively—the Medical, the Surgical, the Obstetrical, the Pathological, the State Medicine, and the Anatomical and Physiological Councils.

21. No Fellow shall be eligible as a candidate for election on more than two Sectional Councils, but no Fellow shall be eligible as a candidate for election on both the Medical and Surgical Sectional Councils. (1888.)

22. Each Sectional Council shall consist of the President of the Section and ten Members, one of whom shall act as Secretary to the Section ; except the State Medicine and Anatomical and Physiological Councils, which shall each consist of a President and six Members. (1888.)

Meetings of Sectional Councils.

23. Each Sectional Council shall meet on a fixed day at least one week before the Ordinary Meeting of their Section, three to form a quorum.

Powers.

24. Each Sectional Council shall have the power of making any such arrangements as it thinks necessary to carry on the work of the Ordinary Meetings which are under its charge, provided that such arrangements do not interfere with the general laws of the Academy ; and any Rules laid down by such Council shall have the force of laws at the Ordinary Meetings under its charge until submitted to the General Council.

25. Each Sectional Council shall have the power of filling up any vacancies that may occur among its Members until the Annual General Meeting.

Committee of Reference.

26. The Council shall appoint a Committee of Reference to report upon morbid growths and other specimens exhibited before the Academy ; of this Committee the Exhibitor shall, for the occasion, be a Member.

Officers.

27. A President, to be elected by the Annual General Meeting in October and to hold office for three years.

28. The Presidents of all Sections shall be elected by the Fellows at the Annual General Meeting, and shall hold office for two years.

29. One General Secretary and Treasurer to be elected at the Annual General Meeting.

30. It is expedient that a fixed salary (of one hundred guineas) shall be paid yearly to the General Secretary in consideration of the fact that the editing of the "Transactions" is part of his duties.

31. One Honorary Secretary for Foreign Correspondence to be elected at the Annual General Meeting. (1888.)

32. The Councillors for each Section to be elected at the Annual General Meeting. Each Sectional Council shall elect two Members to act on the General Council, except in the case of the Sections of State Medicine and Anatomy and Physiology, which shall elect one each. (1888 and 1915.)

33. Two Members in each Sectional Council shall retire annually, and be ineligible for re-election for one year, except in the Council of the Section of State Medicine, and in the Council of the Section of Anatomy and Physiology, in each of which only one shall retire. (1896 and 1912.)

34. Six Secretaries, one for each Section, to be appointed by the Sectional Councils.

35. At all elections after the year 1882, any Fellow desirous of nominating a candidate for election shall, at least ten days before the Annual General Meeting, forward an application to the General Secretary to enter the name of such Fellow on the list of candidates for office, provided that the Fellow so nominated shall have consented to act, and shall have paid his subscription at the time of nomination. Should there be an insufficiency of regularly nominated candidates the vacancies so created shall be filled up by the Sectional Councils at their first meeting after election. (1891 and 1904.)

36. That all elections shall be by ballot, but Fellows residing more than 15 miles from Dublin, and those incapacitated by illness (to be certified), may record their votes by ballot papers, sent to the presiding officer in sealed envelopes provided for that purpose. A Fellow cannot avail himself of this privilege unless his subscription for the current session has been paid. (1896 and 1904.)

37. That in all elections to the Sectional Councils there shall be affixed to the name of each candidate the number of meetings that he has attended of that particular Section of the Council for which he is now a candidate. (1898.)

Duties of Officers.

38. *The President* shall preside at the Annual and Special General Meetings and at General Council Meetings. In the absence of the President, the Chairman shall be appointed by the meeting. (1888.)

39. *The Presidents of Sections* shall preside at the Ordinary Meetings of the Academy, and shall also preside at the Sectional Council Meetings. In the absence of the President, the Chairman shall be appointed by the meeting. (1888.)

40. *The General Secretary* shall attend all General Meetings of the Academy and General Council. He shall take minutes of such meetings, to be read at the following meeting.

41. He shall receive and have charge of all papers intended for publication in the "Transactions" of the Academy, after they have been handed over to him by the Secretaries of the several Sections.

42. He shall, on receiving notice from the Secretary of a Section, send out to all the Members notices of the title or titles of the paper or papers for the next Ordinary Meeting, with the name or names of the authors, and, so far as possible, of the subjects for Exhibition, with the names of the Exhibitors.

43. He shall arrange for the Exhibition of specimens and the reading of papers, which are to be forwarded to the Academy by those who are absent, or are not members.

44. The General Secretary and Treasurer shall receive all moneys, and lodge the same in bank to the account of the Academy, and all cheques shall be signed by the Treasurer and one other Councillor.

45. The Accounts shall be audited by two Fellows, not Members of Council, to be appointed by the President at some meeting previous to the Annual Meeting.

Duties of Secretaries of Sections.

46. To attend the Meetings of the Council of the Section and the Ordinary Meetings of the Academy, under the management of said Council, and to take minutes at such meetings, to be read at the next following meeting of that Section.

47. To keep such papers as the Sectional Councils recommend for publication, for the purpose of handing them over to the General Secretary.

48. To inform the Secretary of the Committee of Reference of any specimens referred to that Committee, and to transfer the specimens to that Secretary.

49. To give notice to the General Secretary, one week previously to the meeting, of the titles of papers for the evening, the names of the authors, and, so far as possible, the objects for Exhibition, with the names of Exhibitors, so that the General Secretary may inform the Members.

Meetings.

50. The Annual General Meeting to take place on the second Friday in October, for the election of Officers and Members of Council, and for the general business of the Academy.

51. Due notice of the meeting shall be given by the Secretary to all Members at least three weeks previously. (1891.)

52. No motion involving a change of these Rules shall be brought before this meeting except one week's notice thereof shall have been given by the Secretary to each Member.

53. The President may—and shall forthwith, on receiving a requisition signed by seven Fellows, at any time—on giving one week's notice, summon a Special General Meeting, for the consideration of particular business, the nature of which must be specified in the letter of summons convening the meeting, and at such meeting no other business can be transacted. In the event of the President being unable, from any cause, or declining, to summon a Special General Meeting of the Academy, it shall be in the power of the General Council to summon such meeting. (1888.)

Ordinary Meetings.

54. The communications to be submitted to the Ordinary Meetings shall be grouped under the following heads :—Medicine, Surgery, Pathology, Obstetrics, State Medicine, and Anatomy and Physiology ; and the conduct of such meetings shall be in the hands of the several Sectional Councils, each Sectional Council to have the management of the Ordinary Meeting in rotation, as arranged by the General Council. (1888.)

55. The Ordinary Meetings shall be held on every Friday evening, from the last Friday in October until the last Friday in May, inclusive, at eight o'clock, except during the Christmas and Easter recesses.

56. All Fellows, Members, and Student Associates attending the meetings shall write their names in the attendance book.

57. Any Fellow or Member may introduce two Visitors by cards obtained from the Sectional Secretaries.

58. Officers of the Army or Navy Medical Departments shall, on presenting their cards, be admitted to the Ordinary Meetings of the Academy.

59. No communication shall exceed twenty minutes in its delivery, nor any speech thereon ten minutes, except by permission of the Chairman. No one shall speak twice upon the same communication, except the author, who has the right of reply.

60. A paper by any other than a Fellow or Member of the Academy shall not be read before the Academy unless the author of such a communication shall have obtained permission to do so from the Council of the Section before which the communication is proposed to be read. (1892.)

Ordinary Meetings.—Order of Business.

61. (1.) Chair to be taken at 8 30 p.m.

(2.) Chairman to read list of specimens, &c., exhibited by card, together with the names of the Exhibitors.

(3.) No Pathological Specimen shall be exhibited at any Section other than the Pathological and Obstetrical, except by card. This Exhibition shall not exclude any subsequent communication regarding it at the Pathological Section.

(4.) There shall be no Exhibition of Specimens by card in the Pathological Section.

(5.) Any member shall have liberty to exhibit any recent specimen at any of the meetings of the Obstetrical Section, provided it illustrates any question in gynæcology.

(6.) At the meetings of the Obstetrical Section recent specimens may be exhibited, and the President may invite discussion thereon, provided that such exhibition of specimens or discussion, if any, thereon, must terminate at 9 o'clock, p.m., but that, if necessary, they may be resumed after the papers for the evening have been read and discussed.

(7.) Chairman to ask if any member has any observations to make or motion to propose relative to any living specimen on the List of Exhibition.

(8.) Chairman to call upon the author of the first paper on the list to read his paper.

(9.) Chairman to call upon members to discuss the paper, or, at his discretion, to take any other paper or papers on the list relating to the subject, and have the discussion subsequently on all such papers collectively.

(10.) When the last paper has been discussed, the Chairman to ask if any member desires to speak upon any of the specimens exhibited by card.

(11.) After the discussion upon any specimen, the Exhibitor has the right of reply.

Regulations regarding the Exhibition of Specimens by Card.

62. (1.) Any member may exhibit by card at any Ordinary Meeting, except at the meeting of the Pathological Section. At the meetings of the Pathological Section all specimens must be presented and described *vivâ voce*, and debate may be invited thereon.

(2.) Notice shall, if possible, be given to the General Secretary, or the Secretary of the Section, on or before the previous Ordinary Meeting.

(3.) Specimens must be in the room at 7 45 on the night of Exhibition.

(4.) Specimens for Exhibition by card shall be open for inspection at 8 p.m.

(5.) A card, containing all particulars for publication, shall be placed with the Specimen. Cards for this purpose are to be obtained from the Secretary.

(6.) The Exhibitor should be present, and he shall furnish further details if asked for.

(7.) Every Exhibitor shall submit the Specimen or Specimens on view to the Committee of Reference, if the meeting so decide.

Exhibition of Pathological Specimens.

63. No lengthened reference to treatment shall be allowed upon any Specimen, except by the express permission of the Chairman. Whenever it has been agreed that a Specimen exhibited at a Sectional Meeting of the Royal Academy of Medicine in Ireland shall be sent to the Reference Committee to report thereon as to its nature, the Exhibitor is to retain the custody of the specimen until he shall be summoned to a meeting of said Committee to be convened by its Secretary, on an early day, when he will attend and submit it for examination. (1889.)

By-laws concerning "Transactions."

64. The "Transactions" shall consist of such Communications made to the Academy by or through Fellows or Members as may be deemed by the General Council suitable for publication; also, of discussions of importance or interest arising out of such Communications.

65. All Communications accepted by the Academy become the property of the Academy, but authors may also print their Communications, subsequent to the reading of the same before the Academy, in any publication in addition to the "Transactions." Papers shall be handed to the Secretary of the Section immediately after they have been read. (1891.)

66. The "Transactions" for the year shall be presented to all Fellows of the Academy who have paid their Annual Subscriptions.

67. The "Transactions" may be purchased by Members at cost price.

68. The Publication Committee of each Section shall meet not later than the Tuesday after each meeting of the Section, for the purpose of abstracting the proceedings—the abstract to be placed in the printer's hands on same evening, and forwarded to the editors of medical journals with the least possible delay. (1888.)

69. Contributors of papers may send their papers to the Academy printer early enough to allow of their being put in type before the meeting, provided the author be responsible for the cost of same should the General Council deem the communication not suitable for publication in the "Transactions."

70. That on the evening of the day of meeting of the Sectional Council when the papers for the next meeting have been decided upon, a circular be sent to each contributor by the Secretary of the Section informing him :—

(1.) That he is expected to be ready or else take his place at the bottom of the list.

(2.) That he must have an abstract ready with his paper, otherwise he will be noted in the published proceedings in such form as the Publication Committee think fit.

71. The General Council is empowered to defray the expenses in whole or in part of any illustrations which it may consider advantageous to the elucidation of the papers published by the Academy.

72. An abstract (prepared by the author) of each communication made at the Academy, along with a report of the discussions thereon, shall be furnished to the editors of such medical journals as may desire to publish them, and the authors of such communications shall be empowered to publish their papers *in extenso* in any periodical or periodicals they may think fit, such communications also to appear in the "Transactions," provided the Council considers them worthy of insertion.

Expulsion of Fellow or Member.

73. Expulsion of a Fellow or Member can take place only at a General Meeting of the Academy, on the motion of the Council, if two-thirds of the Members present shall vote for the same by ballot. Of such ballot the Council must give at least fourteen days' notice in writing to every Fellow of the Academy.

New Laws.

74. New Laws, or alterations in existing Laws, can be proposed only at the Annual General Meeting. Any Fellow proposing such alteration shall give notice to the General Secretary at least ten days before the General Meeting in October.

REPORT.

The General Council reports that the number of Fellows is 144, of Members 17, and of Student Associates 8. The numbers last year were—Fellows 149, Members 16, and Student Associates 5.

The Accounts have been audited and show a balance of £106 6s. 1d. Last year the balance to credit was £43 13s. 9d.

During the latter part of the Session the affairs of the Academy were largely interfered with by the Sinn Fein Rebellion. It was impossible to hold some of the meetings as arranged, and both the attendance and the number of communications was very much reduced, the smaller size of the last volume of the "Transactions" giving tangible evidence of the latter fact.

It was decided by the Council that the funds of the Academy which had been invested in $2\frac{1}{2}$ per cent. Consolidated Stock (£596 2s. 4d.) and in New $2\frac{1}{2}$ per cent. Government Stock (£155 1s. 10d.) should be reinvested in $4\frac{1}{2}$ per cent. War Loan (1925 to 1945). The trustees have since effected this. The amount now stands at £496 16s. 7d., which will give a slightly increased interest after paying commission and stamp duty.

The Council records with deepest regret the deaths of four of our Fellows.

Sir Charles B. Ball, Bt., M.D., F.R.C.S., was one of the original Fellows of the Academy. He was specially

interested in the Surgical Section, at which he frequently made communications and took part in the discussions. He occupied the position of President of the Academy during the years 1909 to 1912.

James Byrne Coleman, C.M.G., M.D., F.R.C.P.I., often read papers before the Academy, especially in the Section of Pathology, of which Section he was President during 1913 and 1915.

Arthur Hamilton White mainly devoted his attention to the Section of Pathology, in which he took a keen interest. He acted for four years as Sectional Secretary and as President during the years 1911 and 1913.

Charles E. FitzGerald, M.D., F.R.C.P.I., one of our original Fellows. His genial manners will always keep him in remembrance by those who knew him.

29*th September*, 1916.

General Treasurer's Report for the Session 1915-1916.

Dr. RECEIPTS.	£	s.	d.
To Balance in Bank	42	10	10
,, Balance in Hand	1	2	11
,, Subscriptions and Entrance Fees	307	11	0
,, Dividends on £596 2s. 4d. 2½ per cent. Consolidated Stock, and £155 1s. 10d. New 2½ per cent. Government Stock [Now invested in £496 16s. 7d. 4½ per cent. War Loan (1925-1945)], and £86 12s. 2d. Bank of Ireland Stock	29	4	2
,, Sale of Transactions	5	0	0
,, Interest on Deposit Receipts	4	5	3
Total	£389	14	2

Cr. EXPENDITURE.	£	s.	d.
By General Secretary	105	0	0
,, Royal College of Physicians	15	15	0
,, Royal College of Surgeons	15	15	0
,, Servants	20	0	0
,, Hire of Lantern	3	4	10
,, Groceries and Milk	12	9	2
,, Purchase of War Loan	3	17	6
,, Sundries and Postage	3	18	9
,, Printing and Stationery	31	7	9
,, Printing Vol. XXXIV. Transactions	72	0	1
,, Balance in Bank	106	2	11
,, Balance in Hands	0	3	2
Total	£389	14	2

The Capital is invested in the names of Dr. Walter G. Smith, Dr. James Craig, and Dr. Henry Jellett.

We have examined the Accounts and Vouchers, and certify the same to be correct.

4*th October*, 1916.

TREVOR N. SMITH, F.R.C.S.I.
HENRY STOKES, M.D.

Volume XXXIV. of the "Transactions" has been forwarded to the following:—

IRELAND:—

Queen's University	Belfast.
University College	Cork.
Do.	Galway.
Medical Press	Dublin.
National Library	Do.
Royal College of Physicians	Do.
Royal College of Surgeons	Do.
Royal Irish Academy	Do.
Royal Dublin Society	Do.
Royal Veterinary College of Ireland	Do.
National University	Do.
Trinity College	Do.

ENGLAND:—

Birmingham Medical Review	Birmingham.
Medical Institute	Do.
Bristol Medico-Chirurgical Journal, Medical Library	Bristol.
The University Library	Cambridge
University of Durham College of Medicine	Newcastle-on-Tyne.
Liverpool Medico-Chirurgical Journal	Liverpool.
Journal of Obstetrics and Gynæcology, 60 Chandos-street, W.C.	London
King's College	Do
Lancet	Do.
Library, British Medical Journal, 429 Strand	Do.
Lister Institute of Preventive Medicine, Chelsea Gardens	Do.
Medical Magazine, 44 Bedford-row, W.C.	Do.
Medical Review, 70 Finsbury Pavement, E.C.	Do.
Official Year Book of Scientific and Learned Societies, Exeter-street, Strand	Do.
Public Health, 1 Upper Montague-street, Russell-sq., W.C.	Do.
Royal College of Physicians	Do.
Royal College of Surgeons	Do.
Royal Society of Medicine, 1 Wimpole-street, W.	Do.
University of London, South Kensington	Do.
University College, Gower-street, W.C.	Do.
The Manchester Medical Society, The Owen's College	Manchester
Victoria University	Do.
The Bodleian Library	Oxford

SCOTLAND :—

University	Aberdeen.
University College	Dundee.
Royal College of Physicians	Edinburgh
Royal College of Surgeons	Do.
The Advocates' Library	Do.
University	Do.
Faculty of Physicians and Surgeons	Glasgow.
University	Do.
Do.	St. Andrews.

WALES :—

National Library of Wales	Aberystwyth

EUROPE :—

University	Amsterdam.
Naturforschende Gesellschaft	Basel.
Kgl. University Library, Dorotheen Strasse	Berlin.
University College	Bologna.
Académie Royale de Médecine de Belgique	Brussels.
Société Belge de Chirurgie	Do.
Université Libre	Do.
Rivista Italiana di Neuropatologia, &c.	Catania
University College	Christiania
Académie de Médecine	Copenhagen.
La Grèce Medicale, Syra	Athens, Greece.
Institute Bacteriologico Camera Pestana	Lisbon.
University College	Madrid.
Archivio di Ortopedia	Milan.
Académie de Médecine, Rue Bonaparte, 16	Paris.
Revue de Chirurgie	Do.
University of Paris	Do.
Imperial University	Petrograd
Karolinska Med. Khir. Institutet	Stockholm.
University Library	Strasburg.
Archivio Italiano di Otologia	Turin
Medical Society, Royal University	Upsala.
University	Vienna.
Naturforschende Gesellschaft	Zurich.

AMERICA :—

Johns Hopkins Hospital, North Broadway	Baltimore, Md.
Journal of Medical Research	Boston, Mass.
McGill University	Montreal.

AMERICA—*con.*

Academy of Medicine, 17 West 43rd Street	New York.
University	Do.
American Medical Association	Chicago.
College of Physicians	Philadelphia.
University of Pennsylvania	Do.
University Laval	Quebec.
University of Toronto	Toronto.
Library, Surg.-General's Office	Washington.

AUSTRALASIA:—

University	Adelaide.
University	Auckland.
University	Melbourne.
Inter-Colonial Medical Journal	Do.
University of Otago	New Zealand.
The Australasian Medical Gazette 32–34 Elizabeth Street, Sydney.	Sydney, N.S.W
University of Sydney	Do.

ASIA:—

Bombay University	Bombay.
University	Calcutta.
Imperial University Library	Tokio. Japan
Library, Bureau of Science	Manilla, P.I.

TRANSACTIONS

OF THE

ROYAL ACADEMY OF MEDICINE IN IRELAND

NEPHRECTOMY.

By LT.-COL. WILLIAM TAYLOR, R.A.M.C., M.D., Univ. Dubl.;

President of the Royal College of Surgeons in Ireland;
Surgeon to the Meath Hospital and County Dublin Infirmary.

[Read in the Section of Surgery, October 27, 1916.]

When going over my renal cases I found that I had performed 138 operations upon 128 patients. The operations consisted of 92 fixations, 26 total nephrectomies, 1 partial nephrectomy and 19 nephrotomies. Of the 19 nephrotomies, 5 were merely exploratory, and 14 were for the removal of calculi (in the case of one patient calculi were removed from both kidneys). There was one death. This death occurred in the case of a gentleman whose left kidney I removed for hydronephrosis. There was nothing whatever difficult about the operation, which was completed in less than half-an-hour. The drainage tube was removed on the morning of the second day, and the wound was soundly healed at the end of a week. On the tenth day I allowed the patient to sit up, on the thirteenth day he was walking about, and had arranged to return home two days later.

There had never been any fever nor increased rapidity of the pulse. Having been up for several hours and feeling—to use his own expression—in the best of spirits, he decided to walk round his room a few times after his supper, before going into bed for the night. Whether he tried to do this I do not know. As the nurse left the room to get a hot jar for his bed, she heard him call, and returned immediately to find him sitting on a chair with his face twitching, and, as she thought, dying. In five minutes he was dead. What the cause of his death was it is hard to state, as he did not complain of any sudden pain, and did not appear to have any difficulty in breathing.

With regard to the different groups of cases, there were 92 fixations in 83 patients, 9 being double nephropexies. The operation of nephropexy is not performed by any means so frequently now as it was some eight or ten years ago, and I think rightly so. It is seldom that a movable kidney which is not actually causing definite renal pathological changes gives rise to symptoms which can be unhesitatingly attributed to its mobility, and unless the patient's symptoms can be definitely ascribed to a movable kidney, I do not think any operation should be undertaken. But, if treatment for the movable kidney is indicated, in my opinion the operation of nephropexy is certainly preferable to the use of a belt. Of the 19 nephrotomies, performed upon 18 patients, 14 were for the removal of calculi (in one patient calculi were removed from both kidneys), one was an exploration of the left kidney for very severe hæmorrhage. The remaining nephrotomies were undertaken in cases where the most typical symptoms of stone were present, but the explorations were negative. The hæmaturia case was that of a lady whom I saw in consultation some six or seven years ago.

For twenty-four hours before I was called in she had been passing large quantities of blood, the hæmorrhage coming on quite suddenly as she was going about her household duties. When I saw her the bleeding was very profuse, and I had her at once removed to hospital, where Dr. Cecil Smyly kindly cystoscoped her. It was with difficulty he could get the bladder sufficiently clear of blood to be able to see distinctly, but, after some time one could see the bleeding coming profusely in strong jets from the left ureter. Morphine and lactate of calcium were given, but without effect, and, as next day the lady was still bleeding freely, and as constitutional evidences of the loss of blood were becoming manifest, I explored the left kidney. On withdrawing the kidney from the lumbar incision nothing abnormal was to be seen or felt. The kidney was then split open from pole to pole, but no gross pathological lesion could be detected to account for the bleeding; there were no signs of new growth, nor signs of tubercle, and there was no stone. In every respect to the naked eye it was a perfectly normal kidney. The surfaces were sutured together by mattrass sutures of cat-gut. The kidney was then fixed by gauze packing and the wound closed. In three days the hæmorrhage had ceased, and on the fourth day the urine appeared normal. She was able to leave hospital in two and a half weeks, and, so far as I know, she has never had any recurrence of bleeding. What is the pathology of this form of hæmorrhage? With regard to the other exploratory nephrotomies I am not quite sure what the pathology was, but I believe three were cases of pyelitis. One was most probably a chronic interstitial nephritis. The former recovered completely, the latter died from uræmic convulsions four years subsequently. Of the nephrectomies 26 were complete

nephrectomies and one was partial. The latter was performed for the removal of a tumour which seemed to be growing from the lower pole of the left kidney in the case of a lady aged about 43 years. The tumour was about as large as the closed fist, and on section after the operation seemed to be of a fibro-lipomatous character. It was apparently intimately connected with the lower pole of the kidney which was removed with it, and the patient has since enjoyed good health. Of the 25 complete nephrectomies, 12 were performed for tuberculosis, 3 were performed for hyper-nephroma, and one for a new growth, believed to be hyper-nephroma, but the nature of which was not definitely settled; 4 were performed for hydronephrosis, and 6 for extensive pyonephrosis. The operation of nephrectomy may be one of the easiest and simplest of surgical procedures, or, on the other hand, it may be one of the most difficult and trying operations that a surgeon could encounter, and may tax his ingenuity and resource to the uttermost. The difficulty of nephrectomy depends mainly upon the extent of the adhesions, and it is in determining whether these adhesions should be completely separated and the entire kidney and its capsule removed, that sound judgment is required. My feeling is that the majority of surgeons do not recognise the great ease and safety with which sub-capsular nephrectomy can be performed. Unless in case of new growth, and even in some of them, the adhesions are due to peri-nephritis which brings the capsule of the kidney, the peri-nephritic tissues and all the surrounding structures into one inseparable mass. I believe any competent experienced surgeon ought to be able to determine within a very few minutes after exposing the kidney whether he can, with any prospect of success and with safety to the patient,

separate the adhesions which are binding the diseased kidney to the surrounding structures. If the disease for which the operation is undertaken is malignant, every effort must be made to remove the kidney and its capsule completely. If in other cases it is obvious that great difficulty is to be experienced, and much time expended, in the separation of adhesions whereby a considerable amount of blood will be lost and shock thereby intensified, I feel satisfied that sub-capsular enucleation should be the operation selected. I have several times removed kidneys by sub-capsular enucleation within a few minutes, without the loss of more than a couple of ounces of blood, and with no appreciable shock, in which cases I am convinced I could not have removed the entire kidney without a tedious operation, without the loss of a considerable amount of blood, and with very severe shock. I know of a case in which two hours were expended in attempting to separate adhesions in order to extirpate a kidney for calculous pyonephrosis, but without success.

My experience convinces me that the operation of sub-capsular nephrectomy is practically devoid of danger. On one occasion I was compelled to adopt this method of removing the kidney in a case of old-standing tuberculous disease associated with secondary pyogenic infection producing an enormous pyonephrosis. The kidney was so fixed that it could not be moved. Sub-capsular nephrectomy was accomplished within a very short time, and beyond the fact that a sinus persisted in the lumbar region for about two months, there was no further trouble, and the patient is now, six years after the operation, in perfect health. It is with tuberculosis of the kidney, I take it, that physicians are most concerned. Urinary tuberculosis is almost invariably primarily situated in the

kidney. Vesical tuberculosis is always secondary to extension from the kidney or epididymis. The diagnosis of renal tuberculosis is not always easy. The early symptoms are frequency of micturition, hæmaturia, pain, and pyuria, and generally they appear in that order. Primary renal tuberculosis should be suspected, and very carefully repeated examinations of the urine should be made with that view in mind, in all cases in which diurnal frequency of micturition seems to come on without other obvious cause, and persists for some time, more especially if associated with pus. Repeated examinations should be made for tubercle bacilli in the centrifuged urinary deposit, and cystoscopic examination should be made by a skilled cystoscopist. The presence of acid-fast bacilli in the urine is not always conclusive proof of the presence of tubercle in the kidney. Even the presence of acid-fast bacilli in a catheter specimen is not absolutely conclusive. I know of some cases in which acid-fast bacilli were found in a catheter specimen, and yet the subsequent course proved that the trouble was not tuberculous. Smegma bacilli may easily contaminate a catheter specimen if great care is not taken in passing the catheter, even after careful washing of the parts, and I must say I do not see why smegma bacilli might not occasionally pass along the female urethra into the bladder and therein produce irritation, and so cause frequency of micturition. In a doubtful case some of the centrifuged urinary deposit should be injected into a guinea-pig.

I have no faith whatever in von Pirquet's test, and I have no experience of Calmette's test. The diagnosis once established, nephrectomy in the early stage of renal tuberculosis is the only method of treatment by which a cure can be assured. It is indicated whenever the

diagnosis of uncomplicated unilateral renal tuberculosis is made. Nephrectomy of the primarily diseased kidney may still be performed in some cases even in the presence of secondary involvement of the other kidney, provided that the functional activity of this secondarily affected kidney is satisfactory. In such a case the operation is undertaken with the object of diminishing the toxæmia. Nephrectomy is contra-indicated in advanced disease in other organs, such as the lungs, peritoneum, spine, or joints, as well as in a generally enfeebled state of the patient, and in any case in which the functional activity of the other kidney is not considered equal to the task of maintaining life. It may be said that cases of renal tuberculosis have recovered under treatment by tuberculin and other medical methods. That such is the case I do not for a moment doubt. In fact, I know of two such cases, but still, cure by such treatment is exceptional and cannot be relied upon. If renal tuberculosis did frequently get well without operation it would be reasonable to expect that we should find evidences of healed tuberculous lesions in kidneys at *post-mortem* examinations, and yet I am not aware that many, if indeed, any, such cases have ever been recorded. That permanent recovery from renal tuberculosis without surgical removal of the diseased kidney is exceptional, and not to be relied upon, is borne out by the report of Wildbolz at the last German Congress of Urology. He collected 316 cases of renal tuberculosis treated in Switzerland, the place above all others supposed to have climatic conditions exceptionally favourable for the treatment of all tuberculous affections. As well as I can remember only 10 per cent. of the patients were alive more than five years after the onset of the disease, and in only five per cent. had all symptoms dis-

appeared over five years. The results of surgical treatment are certainly more satisfactory, and are followed by a cure in the majority of cases operated upon. Since the introduction of modern methods of diagnosis of renal function the immediate mortality of nephrectomy for tuberculosis has fallen to less than three per cent. In one large clinique the actual mortality was 2.85 per cent., and in the Mayo Clinic, where over 200 cases were operated upon, the mortality was 2.9 per cent.

Of the deaths that occur at a later date from tuberculosis in other parts, or from complications which were present at the time of operation, fully 60 per cent. take place within a year. Of my cases, so far as I can ascertain ten patients are still alive, one died of extensive secondary tuberculosis three years subsequent to operation. The other death, which has recently occurred, was that of a young girl whose right kidney I removed nine years ago for tuberculosis, at the age of 17 years. At the time of operation there was considerable bladder infection, and we believed the left kidney was secondarily involved, but still functionating satisfactorily. The bladder ulceration never healed, in spite of tuberculin and local treatment, but this I attribute to the fact that the other kidney was involved, thereby continually infecting the bladder, and that the patient's resistant power to tuberculous infection was extremely low. Her family history was exceedingly bad. One member died of tubercular meningitis, one died of tubercular peritonitis, one of phthisis, and one had tuberculosis of the knee-joint, while the father had also died of tuberculosis. This girl died during the past year. The remaining ten cases are alive and well. Of course, no one supposes for a moment that nephrectomy could possibly increase the resistant power of the patient

or do other than remove the active source of infection. In short, then, everything points to the conclusion that nephrectomy holds out the only hope of assured cure for renal tuberculosis, and the sooner the operation is performed after the operation is diagnosed the better.

With regard to nephrectomy for hyper-nephroma there is little to say. One case is of interest from a clinical point of view. It was that of a gentleman about 56 years of age. Eighteen years before I saw him he noticed that for two days there seemed to be some blood in his urine. It appeared without cause, so far as he knew, and disappeared without treatment. Twelve months before coming under my care he got an attack of pneumonia, which terminated in the expectoration of a considerable quantity of pus and blood. Every week or ten days he became feverish, felt ill, then had a fit of coughing and expectorated a quantity of pus and blood, varying in amount from a few ounces to half a pint or more. This condition continued from October, 1908, until I saw him in November, 1909. At that time there was a swelling extending downwards from the sixth rib, as far as the iliac crest on the right side. The opposite side of the chest in comparison seemed flattened. The diagnosis suggested was a hypophrenic abscess communicating with the lung, and the bulging that extended downwards was taken to be the displaced liver. It was seen by several of my colleagues, and they all concluded that the case was either an hepatic abscess, or a right hypophrenic abcess. *X*-ray photographs and the use of the *x*-ray screen gave us no further information. A few pus cells were found in the urine. Exploratory incision showed us that the liver was perfectly normal, both in position and size, and that the tumour was really

the right kidney. As a free communication existed between the kidney and lung, it was obvious that it would not be judicious to attempt to perform nephrectomy. Consequently, after closing the exploratory incision the tumour was exposed through the loin, and the suppurating mass was incised. A considerable quantity of broken-down tissue and pus escaped, and a quantity was further scooped out with the hand until hæmorrhage became so free that several laparotomy sponges had to be packed into the cavity to stop the bleeding. A moderate amount of blood was expectorated for two or three days, after which the expectoration gradually ceased. The gauze sponges were removed day by day, and in the course of some four or five weeks the man was able to walk about free from cough and expectoration. Two months subsequently, as there had been no pulmonary trouble in the meantime, and as the man seemed in good health with nothing but a small sinus in his loin, I decided to try and extirpate the kidney. This was accordingly done, and was by no means easy of accomplishment as the adhesions were considerable, but recovery was complete, and the patient is now, seven years after the operation, in perfect health. The tumour after removal weighed over 8 pounds, and was a typical hyper-nephroma, growing mainly from the upper poles of the kidney.

Taking all my cases together there were 138 renal operations with one death, or taking them in groups there were 92 fixations without any mortality; 19 nephrotomies without mortality; 9 nephrectomies for calculous hydronephrosis, calculous pyonephrosis, or pyonephrosis without calculi, with one death; 13 nephrectomies for tuberculous disease without a death; 4 nephrectomies for tumour and one partial nephrectomy for tumour all followed by

complete recovery. The conclusions I am inclined to deduce from my experience of these cases are :—

1. That where movable kidney is unquestionably the cause of the symptoms complained of by a patient or, when the mobility is leading to actual renal pathological changes, active treatment is demanded, and I believe the operation of nephropexy is undoubtedly preferable to the use of any mechanical apparatus. The operation is one practically devoid of risk.

2. That calculi once diagnosticated should be removed by operation without delay, otherwise destruction of the kidney will inevitably occur sooner or later. The risk of the operation of nephrectomy, which will then probably be necessary is considerably higher than that of nephrotomy. It may be taken that the mortality following nephrotomy in aseptic cases is not more than 2 per cent., whereas the mortality of nephrectomy for calculous disease is about 17 or 18 per cent.

3. That tuberculosis of the kidney is best treated by nephrectomy performed as soon as possible after the diagnosis has been effected, and if possible before secondary infections have occurred elsewhere.

4. That the prognosis of nephrectomy for hypernephroma, if performed before secondary deposits have occurred in other parts, is exceedingly favourable.

5. That partial nephrectomy is an operation of very limited usefulness, but that under certain conditions it is quite a safe and satisfactory procedure.

IMPRESSIONS DURING A SHORT PERIOD OF ACTIVE SERVICE WITH THE ROYAL ARMY MEDICAL CORPS.

By MAJOR W. I. de COURCY WHEELER, M.D., B.Ch., F.R.C.S.I.;

Honorary Surgeon to the Forces in Ireland.

[Read in the Section of Surgery, October 27, 1916.]

In this paper it is intended to give in a general way my impressions of the Surgical and Medical organisations in France and, while avoiding details, to relate some experiences by way of illustrating the *rôle* of a surgeon on short, temporary duty with the Royal Army Medical Corps. An invitation which the War Office had been good enough to give me on July 20th to visit and inspect the Hospitals in France in a civilian capacity was withdrawn at my request, and instead, a week later, I was ordered by telegram to proceed on duty with the Royal Army Medical Corps. Having reported, and while awaiting orders a visit was paid to the various hospitals in Boulogne.

With the exception of a few hotels, some French shops and residences, the entire town is transformed into a huge base for the wounded. Some of the hospitals, so far as the number of beds is concerned, approximate in size all the hospitals in Dublin put into one.

Innumerable motor ambulances are collected at depôts in the town, convoys of which are seen at all hours of the day and night very slowly carrying wounded to the hospitals on the arrival of the ambulance trains.

A day or two spent at the hospitals is sufficient to

demonstrate the superb organisation which has created a medical service efficient beyond description, the machinery of which runs like a well-oiled engine, without hitch, from the trenches to the base.

A great proportion of the war surgery at home is orthopædic. Stiff joints, divided nerves, various contractures, occasional aneurysms, ununited fractures, conical and misshapen amputation stumps, occupy the mind of the surgeon, and his endeavour, however long the treatment, is to procure permanent and useful results.

In France, a surgeon's care is of a different kind. His work is, first of all, life saving. As, for instance, the rapid flapless amputations in cases of gas gangrene, the ruthless routine excision of wounds and wide removal of the lacerated tissues in order to forestall gas and virulent pyogenic infections.

The management of secondary hæmorrhage demands, as of old, the utmost surgical ingenuity.

The direct transfusion of blood in cases of hæmorrhage and sepsis is gaining favour, and appears to justify the belief that not only is it of advantage in supplying the required fluid, but that blood directly transfused has a beneficial effect in septic and toxic conditions.

There is still a third type of surgical work to be found near the front at the casualty clearing stations, differing as much from the surgery at the base in France as the latter does from the work at home.

The surgeon in a casualty clearing station must be a man of prompt action. He must decide instantly the indications for primary amputation, laparotomy, trephining, &c. On him lies the responsibility if a life is lost through gas infection or secondary hæmorrhage, the result of delayed operation. On him rests the decision as to

whether transference to the base is desirable or will be a fatal delay. While many abdominal perforations and brain injuries may be transferred to a base with safety, others if not operated on instantly lose the only chance of recovery. The difficulty of drawing a border line is obvious, but yet there must be no delay in deciding to which class a case belongs. Thus, the responsibility of surgeons, always great, appears to be unusually heavy for those employed at the casualty clearing stations.

It would be impossible in a general communication of this kind to even mention all the novel surgical procedures which have been evolved to cope with the war wounds as seen in France. Many of the innovations have not yet seen the light of day in the journals, and many procedures are quite revolutionary in character.

The Dakin-Carrel method of treating wounds by the injection of Dakin's solution every two or three hours into the dressings by means of specially prepared rubber tubes has resulted in the possibility of converting violently septic compound fractures into simple fractures in a few days. Each day a film examination is made of the wound, and when micro-organisms have been reduced to a negligible number, secondary closure is undertaken with success.

The administration of a known percentage of warm ether vapour automatically by an elaborate machine attracts the imagination of a visitor to the Harvard University Unit. In other institutions a similar apparatus is extemporised at a small cost, and the result of the anæsthesia has fulfilled expectations. The method is clean, the vapour is warm, and by means of an oxygen cylinder can be administered without attention on the part of the anæsthetist. This is an important advantage where there is a shortage of men in a rush at a casualty clearing station,

for the anæsthetist is frequently called upon to perform venesection for the introduction of saline solution during the operation.

Spinal anæsthesia is largely employed in some hospitals in conjunction with general narcosis, following the principle of Crile. In others, especially at the front, it is used without a general anæsthetic. Here patients are so tired when first brought in from the trenches that they sleep without the employment of ether, provided they are secured from pain by the employment of novocaïn or stovaïn.

Blood-pressure readings and blood examinations were made in every severe case. It was found, *inter alia*, that when spinal anæsthesia was used in a case with a reduced hæmoglobin index, the result of hæmorrhage, an alarming fall of the blood pressure often took place. In cases without hæmorrhage, such did not occur. Again, it was noted that if during or at the end of the operation a patient was turned on his side, a severe fall of blood-pressure followed.

A publication of these investigations is to be made in full, and will show the great value of blood-pressure readings during all forms of anæsthesia.

Chest wounds were, as might be expected, accompanied by hæmato-thorax. *X*-ray photographs showed this condition admirably ; but it was necessary, if an accurate picture of the fluid was to be obtained, to have at least one photograph taken in the erect position. *X*-ray plants were modified in many instances to fulfil this purpose.

Exploratory puncture of the chest was a routine expedient, and the blood or fluid collected was examined by film and culture. If the fluid was sterile, the case was treated expectantly ; if micro-organisms were found, even apart from clinical indications, the pleura was usually

drained. In some hospitals siphon drainage was insisted upon. The tube in the pleural cavity was secured to the chest wall by rubber dam or accurate stitching, so as to be air-tight. A second tube, carrying a stream of water from a douche, was connected by a "T" piece with the tube in the chest. It was the familiar device often used at home for vesical drainage. In other units, however, it was pointed out that an air-tight drain in the chest could not be maintained for more than two or three days, and on this account siphonage was not attempted. The treatment of foreign bodies in the knee-joint with bad infection was a subject of much controversy. It was, of course, recognised that drainage was impossible, and if attempted was disastrous.

Mild infections were treated by extension, aspiration and injection of glycerine and formalin, or ether, as is the custom at home.

Foreign bodies without infection were, as a rule, removed and the joint closed ; but in the severe cases of injection the choice appeared to rest between primary excision and amputation. Excision was performed for the purpose of drainage, and the tibia and femur were kept wide apart by weight extension. When sepsis subsided the bones were allowed gradually to come together, and ankylosis followed.

One naturally felt some misgivings as to the final stability of a joint so treated, but it was generally a case of "Hobson's choice."

Not infrequently a wound involving the knee-joint was excised with the surrounding capsule and synovial lining, without drainage. This operation succeeded in selected cases.

Fractures were, as a rule, treated with some modification of the Thomas's splint, with fixed extension.

One wonders why it needed an European war to see the value of the teaching of Robert Jones and the futility of the many wooden arrangements, purposeless in design, which we used to treasure under the name of splints.

Having seen the work of others for a week at the base and become more familiar with military system, we received orders to join the staff of a large general hospital, where the surgeon in charge of the division was, owing to an accident, confined to bed. Ten minutes after arrival we were operating on a case of acute appendicitis.

When not at high pressure, there is a normal amount of "civil" surgery to be dealt with in these hospitals. Thus, we spent days with cases differing in no way from the routine hospital practice at home.

Many hernias, varicoceles, chronic appendix cases and various minor bony ailments were disposed of with local anæsthesia. In one case of hæmorrhoids we departed from the routine infiltration of novocaïn, and passed the needle upwards and backwards into the sacral canal. We injected 25 cc. of 2 per cent. novocaïn solution, and complete anæsthesia of the anal region quickly followed.

There was no difficulty in inserting the needle just above the junction of the coccyx with the sacrum. The proper site for the insertion can be easily felt with the finger, and the curve of the sacrum limits the introduction of the needle to the necessary level.

In the case of left-sided cerebral hernia with right-sided paralysis and aphasia, I had some success by doing a decompression operation on the opposite side. These cases, as a rule, are treated by repeated lumbar puncture under general anæsthesia, the pressure of the cerebro-spinal fluid being carefully estimated by a simple device at each operation. Locally, the hernia is often painted with pure formalin or alcohol.

The daily life in this hospital being novel was of great interest. Military system and discipline and routine administrative efficiency were fascinating to one accustomed to the topsy-turvydom of private surgical practice. The medical officers were for the most part newly-joined civil practitioners of considerable reputation. They and the commanding officer were proud of a unit which ranked high amongst the hospitals in France. Attached to this hospital was one of the famous "jaw huts," where the treatment of fractured jaws and the reconstruction of shattered faces, after appalling wounds, was conducted along lines approaching the miraculous.

It was part of my duty to see these cases from the purely surgical point of view. Thus, Major K., who was responsible for the artistic reconstruction of faces by mechanical ingenuity, passed on his cases to me for the treatment of secondary hæmorrhage and for any necessary plastic operation.

No mental picture can be formed of the appalling secondary hæmorrhage which follows in these cases. Scarcely a day passed without a catastrophe of this kind occurring in the "jaw hut." The patients were pitiable to behold. The features of some blown out of recognition by hand grenade, trench mortar, or other destructive engine. The ingrained dirt, which by a gradual process often took weeks to remove ; the mouth and anterior and posterior nares often thrown into one with a common opening on the surface ; the lips only remaining attached to the face by pedicles of skin ; and the lower jaw in five or six pieces. It is to such a case that one is hurriedly summoned to check the bleeding from the facial, lingual, dental and other arteries. The administration of an anæsthetic under such conditions is better imagined than described.

The ligature of the external carotid, undertaken under great difficulties, as often as not only partially controls the hæmorrhage, and when the operation is temporarily successful bleeding often recurs within the space of a few minutes. Tying of the common carotid artery is followed by hemiplegia in a number of cases. Thus, it can be seen that the question of hæmorrhage in these cases is one of great perplexity. The exposure of the common carotid and compression between the finger and thumb often give sufficient time to deal with the bleeding locally. The common carotid was encircled with a piece of loose catgut with long ends, and the wound in the neck closed, leaving the catgut buried. If hæmorrhage recurred the wound was quickly opened, the end of the catgut found, and the artery tied.

There were five cases of local tetanus treated in this hospital during the time I was attached. The condition is a curious one. Thus, a young soldier wounded superficially in the back near the upper border of the scapula with a piece of shrapnel developed tetanus in the corresponding arm. On approaching the bedside the muscles of the arm violently contracted and stood out, as if trying to lift an enormous weight above his head. There were no constitutional disturbances whatever, and the man, but for this local condition, was in perfect health. Thus, it can be seen that an extraordinary amount of interesting surgical material filled the hospitals at the base.

For about a fortnight I had the privilege of working at and seeing the work of a casualty clearing station. It was a hut hospital, capable of accommodating some 300 patients. I expected to find a place where everything surgical would be improvised ; where the methods would, of necessity, be rough and ready, and instead, I found a well-founded

hospital, staffed by surgeons of exceptional skill, and equipped with all that modern surgery could require.

One tent was devoted to the immediate treatment of gas cases, and oxygen could be administered to 30 or 40 patients simultaneously from the same cylinder, by an ingenious arrangement improvised by the commanding officer.

The results of operative treatment were striking. Thus, a case with multiple abdominal lesions, necessitating a resection of intestines, suture of the stomach and exploration of the kidney, with a compound fracture of the femur thrown in, would the following day appear bright and well.

Here are examples of the type of cases to which I refer :—Private D. (Canadian), admitted on August 28th. Fracture of the lower jaw, fracture of both forearms, abdominal tenderness, and suspicion of gross intra-abdominal lesion. Catheter specimen of urine contained blood. Abdomen became more rigid in a few hours, but pulse remained between 80 and 90. No vomiting. Following morning patient's aspect worse. Rigidity more marked ; vomiting had commenced, and was repeated several times ; general appearance suggestive of peritonitis. Patient unable to pass water since admission, but catheter passed every four hours, resulting in the withdrawal of quantities of urine, varying from a pint on the first occasion to a few ounces at other times. Blood was present on only the first occasion. I operated on August 29th, with the assistance of Captain M. A large rent was found in the upper surface of the bladder, the addomen was full of urine, without odour. The rent was stitched by layers of catgut and the abdomen closed with suprapubic drainage. On each occasion the catheter had gone through the rent in the bladder, and the urine had been drawn from the general peritoneal cavity.

The first diagnosis on the admission of this patient was ruptured bladder, but the result of catheterisation unfortunately led to uncertainty and delay.

Notwithstanding the multiple injuries and the delay of twelve hours before operation the patient did well.

Sergeant H., Welsh Fusiliers, was admitted on August 26th. He had been hit by a "whiz bang." He had a clean scalp wound beneath his steel helmet, running transversely across the region of the superior occipital curved line. He was conscious and bright, and but for the fact that he was blind an intra-cranial lesion would scarcely have been suspected.

The patient was anæsthetised with warm ether vapour, as was the excellent custom in this clearing station. The scalp wound was excised in the routine fashion in order to lessen the danger of gas gangrene, and the bone was freely exposed. A very small depression could be seen to the left of the occipital protuberance, and an almost imperceptible fissure ran obliquely through the bone downwards and to the left. The skull was rapidly opened by Hudson's drill, and the opening enlarged for several inches with nibbling forceps. There was no visible pulsation, and the dura was purple in colour from subdural hæmorrhage. On opening the dura the brain was found pulped, and with the relief of tension literally flowed out of the wound with clotted blood. Quite a large portion of brain was helped away with the aid of a spoon. The lateral sinus was wounded, and two forceps were left *in situ* for two or three days in order to control the resultant bleeding. The interesting points about this case were the absence of any compression symptoms in the presence of marked intra-cranial tension ; and secondly, the rapid recovery of sight after an operation in the occipital region entailing the loss of much brain

matter. The day after fingers could be counted, the next day articles held before him could be recognised, and his recovery was progressive at the time I left.

With thousands of cases passing through it is difficult to even touch on the great variety of wounds requiring treatment. At home we see the wounded only after elimination of the most acute cases, in a casualty clearing station every case is "an emergency," and each case, the wounds being frequently multiple, represents half-a-dozen emergencies.

The results of surgical treatment are impressive at the base, but they are astonishing at the front, even though the mortality is of necessity high. The widest resection of intestines, the high amputations at the hip, and lacerated wounds of the lung often run a course which never gives a moment's anxiety to the surgeon in charge. Unless a man is moribund, his life is never despaired of, and with ever so little help the most severely wounded seem to make for recovery in a manner unknown at home.

It must not be thought there is no recreation for those on surgical duty in France. Even with heavy casualties, the work being all concentrated in one place can be rapidly finished. The orderlies are highly skilled in the preparing of patients for operation, and are, in addition, skilled theatre "sisters."

My visit to France concluded with an inspection of two field ambulances between the clearing station and the trenches, a collecting station, and an advanced aid post in the trenches. We made the journey equipped with gas helmets and steel helmets in an armoured ambulance as far as it was possible to travel over shell-torn roads. I was told that, notwithstanding the craters over which it seemed daring to proceed, that convoys of motor cars were

driven each night without lights over these same roads by drivers who could instinctively locate every dangerous crevass.

In conclusion, I would like to record my feelings of pride at belonging, especially in an honorary capacity, to a Corps which has accomplished such wonders in the present war.

Admiration of all I saw has, I fear, induced me to trespass on ground which by right belongs to those medical men in civil practice who have sacrificed much by prolonged service in the hospitals abroad.

ON KIDNEY DISEASE.

By HENRY C. DRURY, M.D. Dubl., F.R.C.P.I.;
Physician to Sir Patrick Dun's Hospital;
President of the Section of Medicine in the Royal Academy of Medicine in Ireland.

[Read in the Section of Medicine, November 3, 1916.]

I FIRST wish to express my thanks for the honour you have done me by electing me President of the Medical Section of the Royal Academy of Medicine in Ireland.

I appreciate the position very highly, for in my own medical lifetime I have seen this Chair occupied by those alone, whom I have looked upon with respect and honour, my teachers, and my friends, in the foremost ranks of the profession in this country. Some cynic amongst us may say, "Oh, you have been elected to the position because there was very little choice." Unfortunately, our cynic is quite truthful in this, and I acknowledge it with regret. I have attended the meetings of the Section with fair regularity ever since I was qualified, for I felt that I not only received benefit from them, but I considered it a duty to show some interest in this, our representative gathering of the Profession in Ireland. I fear there are too many who do not take, or do not act on this view, and hence the supposed remark of our cynic is too true. I see little else for which I merit the position in which you have kindly placed me.

I think we must confess that the meetings of our Section are not attended as they should be. I hope that

during my year of office we may improve in this matter, and would, therefore, urge on all who are present, that each will make a personal endeavour, not only to come himself to the sectional meetings, but to induce some one else to come also. Why not ask a medical friend in the suburbs to join you at dinner, and come afterwards to the meeting of the Academy? This, in itself, will be a welcome improvement, it will be an encouragement to those who prepare and bring down communications, and stimulate others to do the same. Incidentally it may induce some to join our Society. Let each one also cast about for some subjects to bring forward for information and discussion. There is no necessity for these to be out of the way in either rarity or prolixity. The subjects of everyday practice afford ample material for enlightenment, and brevity in their presentation will add to their interest, and stimulate and give time for their discussion, which will be within the scope of all.

In thinking how we can make our meetings more popular—that is, more successful—one begins by asking: Why are they not more popular? In this connection some unpleasant ideas arise which one would hope are not correct. Are we getting too self-indulgent; too comfortable after dinner to come out to our Medical Society; allowing comfort and laziness to overcome our duty to ourselves, our Academy and our professional friends who have prepared subjects for discussion? We almost fear there is some truth in this idea.

Another unpleasant thought suggests itself, and I fear very much there is some truth in it also. Are we becoming less professional and more mercenary? I mean, are we, as a body, getting to think less what we can make *of* our profession, and more what we can make *out* of it? If

there is a tendency, as we read the notice paper of the meetings, to think less whether we can learn anything to help us in our work, or give the benefit of our experience to others, and so help them; and subconsciously to think more, will it pay me? I cannot get into the limelight before the public here, this will not bring grist to the mill, so it is no use to me. If there is any truth whatever in it, this is a deplorable condition of affairs. I am sure there have always been in the profession individuals, whose sole idea was to make it pay—they are mere traders, not professional men. I do not refer to these, but to the fear that the profession as a whole is gradually becoming infected with this spirit.

That this was not the spirit of the profession here 50 to 100 years ago, is proved by the proud position to which they raised it in the eyes of contemporaries and later generations, the world over. They won this proud position not by taking their ease in the evening and gathering in money by day—but by starting Societies such as this—indeed, the parents of this—by which they might sharpen each others wits, glean from each others experience, vie with each other in observation and investigation, and enlighten one another by gathering together scattered threads and weaving them into an organised fabric. This is the true professional spirit. But if anyone harps back on the old degrading cry, does it pay? the answer comes with no uncertain voice, Yes! it pays. Though it may not be given to any of us to be revered by future generations, to be honoured through far distant countries, to stand in imperishable marble, amongst our successors in the flesh, all down the years—still, there are compensations for us. Where can you get anything for £. s. d. comparable to the pleasure, the satisfaction, the com-

panionship, the healthy unselfish rivalry, to be met with in your own professional circle—not in the narrow cliques of it—but in the big circle which embraces all members, with all their diverse characteristics, and all their various aims? Cast your mind back and around you, and see if it is not the men who have taken most advantage of these opportunities, who, if not the wealthiest, have been the leaders of the profession, its representatives most respected both by ourselves, and by their fellow-citizens at large. This is payment which cannot be reckoned in gold.

If you agree with me in this line of thought, and act on it, our meetings will certainly improve.

There is still another reason, in my opinion, why our meetings are not better supported. The menu has a tendency to remain heavy and monotonous. We want occasionally an entrée, savoury, and sweets, and if the "trifle" is sometimes "seasoned" so highly that it causes such excitement that outsiders might—quite erroneously—describe it as a "row," it may attract other outsiders for the next night, in expectation of another "row"—for who can withstand the attraction of even a dog fight!

We must endeavour to attract, not only men occupying hospital posts, but other less fortunate practitioners. These are the very men who should benefit most by our meetings, and who would benefit us greatly by giving the results of their own shrewd observation, and very varied experiences. We must admit that we do not attract these men, and ask ourselves—why? If we can agree as to the reason, we are bound to apply the remedy. We have endeavoured to popularise the meetings by holding them in a smaller room, and by allowing smoking—with, however, scant success. The large half-empty hall, and more

formal atmosphere unclouded by tobacco smoke, were not, therefore, the sole reasons for non-success. Can it be that the material we offer does not attract? I fear so.

It is necessary, therefore right and proper, that papers dealing with recent advances in theory and practice, and original investigations of cases or classes of cases, should be brought forward here, and the more elaborate the care bestowed on them, the more valuable they will be, and the more highly should we esteem them.

In presenting such at a meeting some condensation is not only allowable but advisable, the main facts then appearing more clearly, and being more likely to be grasped by the hearers than if they are shrouded in a mass of detail. Such details should be found in the published transactions, where they can be studied and compared at leisure, by all interested in the subject at home or abroad—but their recital at a meeting serves no useful purpose, they cannot be carried in the mind of any ordinary person, so they only confuse and tire an audience, and take up time which might more profitably be devoted to discussion.

Let us take an instance—and in doing so I must assure you that I have no particular example in mind—it is merely a sample of the group of highly technical papers of which we hope to have several. It is of the utmost importance that the changes in the blood in a case of Leucocythemia, under treatment, should be accurately noted and recorded, but the recital of these changes in detail, observed day by day, or week by week, will convey far less to a listening audience, than a short summary of the results, and if such recitals are of frequent occurrence they will make men prefer their fireside to our meeting. It must be remembered that most of the audience cannot

carry out these investigations, but they can judge the results, and they want to do so, and to adopt or reject certain treatments on those results. Our papers should be as brief as is consistent with thoroughness. A long paper is not necessarily a good paper, and it is a poor compliment to the intelligence of his audience for the author to think that they will be impressed by its length.

I can recall papers read before this section, which occupied the whole time of the meeting, but which held the attention of every listener all through, because of their varied and absorbing interest. Others who occupied a like period of time, in which one certain form of treatment was introduced by an elaborate prologue, brightened only by gymnastic exercises over unpronounceable names, introduced for the purpose of impressing everyone, but failing to impress anyone; followed by a long procession of cases, detailing with iteration and reiteration the wondrous results of treatment obtained in each and all. The whole thing a really competent man would have disposed of in five minutes. Such papers *may* have a vivifying effect on the author, but *do* have a deadening effect on our Society. I think that our Council should have some control over such cases. Authors of papers should be required to state explicitly to the Secretary how long they required for their papers. The Council should then discriminate, and allot to men of the first kind the whole time they require; and to men of the second kind, time sufficient, but no more, for a succinct statement of their case. The Chairman of the evening would then be responsible for curtailing exuberance or giving latitude to merit.

Besides the technical papers we must have others less technical, less elaborate, dealing with everyday cases, illustrating difficulties, suggesting lines of treatment or

investigation, and so promoting discussion. Papers of this kind can be prepared by anyone, no matter how busy he is, no matter whether he has a hospital or not. They are suggested in our daily work, thought over as we go to and fro in it, and jotted down in the spare intervals of it. They will benefit the writer, attract and interest the hearers. They will widen the circle of thought, clear up some difficulties, perhaps open up fresh ones—perchance one of them may mark an epoch. These will prove the savouries and sweets of our menu. Well seasoned trifles will occasionally appear and provoke much entertainment. Discussion will be more frequent—even more lively, and there will be no need to go into the highways and ditches to compel men to come in that our house may be full.

What I wish to drive home is, that it is not necessary to wait till some extraordinary or rare case comes into our practice, to enable us to make a communication here. Abundant material is to be found in every man's practice if he has eyes to see, the mind to appreciate, and the wish to improve on it.

Up to the year 1827, every medical man saw plenty of cases of dropsy, and most of them were satisfied that it was just dropsy, and that was all about it. But Richard Bright saw in this common and apparently uninteresting condition, much food for thought, and the thinking inspired investigation, with the result that he made an epoch in the history of Medicine and insured for himself an honoured name down through future generations, by his paper an Nephritis.

Eighty-nine years have elapsed since Bright introduced this new-old disease to the profession; yet how much further on are we since Bright laid down his pen nearly ninety years ago? The simplicity of his method of

diagnosis, and the very common occurrence of the disease which it revealed, would almost seem to have impeded investigation. Just as the older practitioners were content with the diagnosis of Dropsy, now we seem to be content with the diagnosis of Bright's Disease, and there is nothing more to be said about it. Even our hospital class will see nothing more than the diagnosis and very little in that, for when we try to talk to them about the disease they think they can manage the "parenchymatous" and "interstitial" shibboleth, and that this is all they require. They would remain with us if we brought them to a case of Myasthenia gravis, but not having something strange sounding at hand, but only some "common" case they slip away for the more intellectual entertainment of a surgeon amputating a little finger—thus showing the real way to cure.

Last session one of our senior members, Dr. Walter Smith, did not think it beneath him to ponder over and investigate the old subject of dropsy, and he treated us with an illuminating and scholarly paper, pregnant with ideas, which may be fruitful in clearing up many perplexing questions. But though he did not lay down any definite conclusions, I consider it was one of the most important papers we have had for a very long time.

Again, I inquire, how much has our knowledge really advanced on the subject of chronic nephritis since Bright's day? In minor details, something. In general principles, little.

The true aim of medical treatment is, prevention, and this entails a knowledge of the cause. If we get ague, we take quinine and find relief; this was, till a few years ago, purely empirical treatment, and is now only palliative, because the cause has been found. We kill the mosquito

or prevent it biting us, thus curing by prevention—a good and true treatment.

What about the cause of Bright's disease? and what investigations are made to elucidate it? Nephritis glides stealthily through our communities of rich and poor alike, with relentless assiduity. It claims its victims without sign or warning, makes sure of a firm hold before it makes itself felt, and then, in spite of our best endeavour, holds on still. It counts its victims by the thousand.

The Registrar-General has kindly furnished me with the following figures. In 1912 chronic nephritis was given as the "cause of death," in the Dublin Registration district alone, in 209 cases, or 2.39 per cent. of the total deaths in that district, that is, 0.52 per 1,000 of the estimated population of the district. In 1913 the figures were 253 deaths, 2.92 per cent. of total deaths, and 0.63 per 1,000 of population. These large figures do not, to my mind, represent anything like the mortality due to this disease many deaths, such as apoplexy and other conditions, being due to it.

What are we doing to stop this terrible mortality, this fearful scourge?

If it came upon us with a shout, striking but one here this week, another there next week, and so on, laying them out in ghastly disfigurement, or racking pain, finishing them off in as many hours or days, with as great certainty as it really does in years, it would arrest the nation's attention, provoke its horror, and arouse it to demand that something more should be done than we are content with now. Why should we wait till we are spurred or shamed into action?

Yet, how far have we got on with the preliminaries of the fight? What do we know of the cause? Practically

nothing. Gout, lead, alcohol, syphilis, the acute exanthems—these sum the causes we know of. But how do they act? All are so dissimilar, and how few, how very few of our cases can be accounted for by these most crude causes? But how very many, none of these will explain at all.

Would it not be a splendid achievement and a triumph for our Academy if one of our members, through careful investigation, found a cause for many of these unexplained cases; while another worked out how this cause acted, and a third proved how it could be avoided? All the steps which lead to these results having been submitted to the friendly and helpful criticism of our most advanced and experienced members, before being given to the profession in general.

In this mysterious disease, also consider its protean and conflicting forms; its distressing symptoms here, and its unassuming mastery there; its tardy progress or its sometime lightning stroke; and its cynical indifference to our endeavours to curb its general effects or control its special and varied manifestations.

I have only endeavoured, by taking this example of a very common disease, to show that none of us have the excuse of lack of material, for failing to make communications here. Communications which may be instructive, stimulating, of far reaching utility, and incidentally of great benefit to the Academy.

Forgive me if I appear to have been pedantic, to have been lecturing on duty, and talking platitudes. Introspection is often a benefit to individuals, to societies or to communities; and it tells me that I have been wanting, in the directions I have here indicated, and am wishful that you will not follow that example

REMARKS ON THE ALLEN TREATMENT OF DIABETES.

By GEORGE E. NESBITT, M.D., F.R.C.P.I.;
Assistant Physician to the Richmond Hospital, Dublin.

[Read in the Section of Medicine, November 3, 1916.]

The presentation of this paper, founded as it is on a recent and still superficial acquaintance with the subject, is entirely due to the kind encouragement of the members of the Council of this Section, who assured me that even if they learned nothing from it, the subsequent discussion would probably be profitable. On these terms I venture to ask the experience or opinions of others, and to sketch briefly the details of the treatment for those who may not yet have had an opportunity of trying it.

It will be agreed that our treatment of diabetes has hitherto left much to be desired both in theory and practice. F. M. Allen, of Rockefeller Institute, New York, realising our deficiencies, set to work on an elaborate experimental research in which he attempted to reproduce, by removal of portions of the pancreas in dogs, a condition closely resembling true diabetes as seen in the human subject. The results of this work are given in his book, *Studies concerning Glycosuria and Diabetes*, published in 1913, and in numerous American papers and journals since that date. Without entering too deeply into the technical aspect of the subject, it may be stated that he succeeded in obtaining

every grade of apparent diabetes, and it was from the behaviour of these animals under various experimental conditions that his deductions were drawn as to the right line of treatment.

"In the milder types, the measures ordinarily used in human diabetes—namely, restriction of carbohydrate or protein and brief fasting if necessary—suffice to keep the animal sugar-free and in good condition indefinitely. But in a more severe type these measures do not suffice, and the animal quickly passes into a hopeless condition if more radical treatment is delayed. In this type the initial fast must be measured in weeks rather than in days. The subsequent diet must be such as to keep the animal at a low level of weight and metabolism. Certain influences which increase either weight or metabolism tend to bring back glycosuria in these animals as in human patients. If glycosuria persists in any animal the result is a downward sequence of lowered tolerance, emaciation, weakness, cachexia and death, with parallel degenerative changes in the Islands of Langerhans. If glycosuria is prevented, the animals may remain lively and strong though thin; they may improve somewhat with time and the islands seem to be spared. This evidence supports the hypothesis that one set of influences overtax and injure the internal function of the pancreas and the opposite set of influences protect and rest the internal function of the pancreas." (Trans. Assoc. Amer. Phys., 1915. P. 323.)

This extract really sums up the *rationale* of Allen's treatment, which may be divided into three stages—(1) prolonged fasting; (2) estimate of the patient's tolerance for the various classes of foods (the test being the re-appearance of glycosuria); and (3) after-care—much of which can be carried out by the patient who is taught

the underlying principles—the essential aim always being freedom from sugar.

The main points of these three stages must be further elaborated. (1) Fasting. This is not new, as Allen points out, but the method of application is. For a couple of days after admission the patient remains on his usual diet, during which time complete examination of the urine, &c., is made. On the third day the patient is put to bed at absolute rest, given absolutely nothing except 3-4 ozs. of black coffee, clear tea, or meat extract, every 2-3 hours, and water to taste. If acidosis or coma threatens ½ to 1 oz. of whisky is given every 2-4 hours, or it may be given merely to relieve the strain of the fast, which is continued till the patient becomes sugar-free (2-10 days).

This severe initial fast is the most radical departure from accepted custom, and is the most probable deterrent from a trial of the method. A very large number of recorded cases shows, however, that with reasonable care it is perfectly safe, and by no means so trying to the patient as might be imagined. Even, according to Allen, "dangerously weak and emaciated patients have borne the fasting with apparent benefit, giving the impression that they had been suffering more from intoxication than from lack of nutrition." He says that no contra-indication has been met, unless it be the appearance of nausea, vomiting, and prostration. One woman died in such an attack, but had previously shown similar attacks. A man who began to vomit was fed, the symptoms passed off, and when he was again starved the glycosuria disappeared.

If serious symptoms of any kind threaten, which is rare, all authorities give food, and resume the fasting on the first opportunity. It is generally agreed that the clear-

ing up of the glycosuria by this method is a very simple matter, and in my case it presented no difficulty.

A very remarkable fact in this connection is that acidosis will also disappear, except perhaps in the most severe type of case (Hamburger). This is, of course, the exact opposite to the behaviour of the normal subject, who develops acidosis on fasting.

In the severest type of case some modification of the above plan will probably be necessary. Joslin's practice in these cases is first to reduce the fats at the rate of 25 grms. per day, during which time the patient should get no bicarbonate of sodium ($NaHCO_3$), or, if he is getting it, it should be reduced 10 per cent. daily. The other salts should not be omitted. He considers that by the time fat is eliminated, acidosis will probably have disappeared. Protein is then got rid of in three days, and carbohydrate somewhat more slowly. One day's fast will then probably suffice to render the patient sugar-free.

(2). The second stage of graduated feeding is the most troublesome, but also the most important. Its object is to ascertain the patient's tolerance, not only for carbohydrate but for protein and fat. The precise details vary slightly according to different authors, but the process is well summarised by Joslin of Boston, who has a very large experience of the treatment as follows :—

Carbohydrate: When 24 hours' urine is sugar-free, add 150 grms. of 5 per cent. vegetable, and add 5 grms. carbohydrate daily up to 20 grms., then 5 grms. every other day, passing through vegetables, fruits, potatoes and oatmeal to bread, unless sugar appears or tolerance reaches 3 grms. carbohydrate per kilogram body weight (1 kilo = 22 lbs.) (10 st. = 64 kilo.).

Protein: Urine sugar-free two days, add 20 grm. protein

(eggs), and then 15 grm. daily as meat up to 1 grm. per kilo.

Fat: Not given till protein reaches 1 grm. per kilo., then add 25 grm. daily till patient ceases to lose weight, or receives not over 40 C. per kilo. (*i.e.*, 2,560 C. for a person of 10st. weight).

On these lines satisfactory progress will be made up to a point at which sugar re-appears. The real crux is, how to proceed then. The directions are, however, quite explicit—fast for 24 hours, or until sugar-free. The diet preceding the re-appearance of sugar is resumed except that carbohydrate should not exceed half the former tolerance till the urine has been sugar-free for two weeks. In all cases a weekly fast day is recommended, the severity of the fast depending on the carbohydrate tolerance. If under 20 grms., a complete fast; if 20-50, half rations; over 100, half carbohydrate. Most of the cases will probably fall into the second group, entailing half diet every seven days.

Under this regime there is a loss of body weight, and Allen lays stress on the fact that this is not only necessary but beneficial. He considers that we have made a mistake in trying to keep up the nutrition of these patients on the ground that increase of weight or metabolism increases strain on the pancreatic function. As Cammidge puts it in an interesting article in the current number of *The Practitioner*, "one of the aims of the treatment is to make the patient a physiological fit for his pancreas." This article also gives some very useful tables of food values expressed in simple workable fashion.

(3.) The third stage comprises the education of the patient in living up to, or rather down to, his food tolerance. The patients are taught to test their own urines, or, if this is impracticable, to have them examined at

short intervals. It is said that the patients quickly learn to check their diet, and in the two years or so which have elapsed since the introduction of the method, large numbers of patients, under the care of numerous writers, have remained free from sugar. In fact a perusal of the literature gives one the impression that there are no failures!

This brief sketch of the purely practical aspect of the treatment scarcely suffices to make clear the points in which it differs from previous practice. These are summarised by Allen in the article already quoted, and may be enumerated. (1.) The policy of insisting on prompt and lasting freedom from glycosuria and acidosis in all cases; (2) the thorough nature of the initial fast; (3) a subsequent diet such as to keep glycosuria and acidosis permanently absent; (4) a low level of weight is advantageous; and (5) fat feeding may be in itself injurious.

Two important considerations will present themselves in forming an opinion on the merits of such a revolutionary therapeutic scheme; first, is it practicable to carry out, and can we keep the patients sugar-free? And secondly, is the patient better when free from sugar. I regret that my own experience is so little as to be almost valueless—indeed I feel considerable reluctance on that ground in bringing the matter forward.

From the testimony of numerous competent and apparently critical workers, as well as from the experience of the case just shown, it appears quite feasible to render the large majority of cases sugar-free. For, at any rate, two years they have been kept free, and lapse of time alone will tell how long they can remain free.

The second query is not so easily answered. The patient in the sugar-free state is undoubtedly on a somewhat lower

plane as regards bodily activity, and complains of being easily tired. Hamburger quotes the case of a physician of 50, who, when sugar-free, loses weight, looks badly, and complains of exhaustion. When allowed a generous diet he feels in every way better. The freedom from danger of complications should, however, be of considerable weight in deciding to endeavour to become sugar-free. Joslin says: "If you follow this plan of treatment there is little doubt but that you will live longer, only you must live at a reduced rate." This problem is of classical antiquity, and is not confined to diabetes.

The services of an intelligent and conscientious nurse are essential, as the patient's future will be founded mainly on the ascertained food tolerance. The patient's co-operation must also be reliable—surreptitious snacks will completely upset the accuracy of the test. In both these respects I was fortunate.

Two queries, recently put to me, may be touched upon. Would this treatment be advisable in surgical cases before operation? There is little doubt that the safety of the operation would be increased by freedom from sugar and acidosis, and as this condition can be attained it would seem very desirable. The point about which tolerance is reached would seem a very good time, but the upset and apprehension of the operation might be expected to cause a return of sugar in a proportion of the cases.

The other query concerns the question of pregnancy. Joslin says that "owing to the apparently easy control of diabetes, several diabetic married women have seen no reason why they should not become pregnant. So far (May, 1915), I have treated but one case under the new regime, and that patient did well. How other patients will do I cannot predict."

The treatment does not pretend to be the last word with regard to diabetes. In reading the accounts of those best qualified to discuss it, *e.g.*, Allen and Joslin, one is I think, impressed by the fact that they make no extravagant claims. They admit that the success or otherwise of the method depends on a yet unascertained factor, namely, "whether diabetes is an inherently progressive disease, or whether it is the simple weakness of a metabolic function." If the former, this line of treatment would be obviously useless, and the patients would go to the bad and die, though not of coma. "If the latter, downward progress may be indefinitely prevented." They have not so far observed this downward progress.

Allen concludes one of his papers as follows :—"Whatever the ultimate outcome two conclusions seem justified by present knowledge : (1) that this treatment removes glycosuria and acidosis more quickly and surely than has been the practice heretofore; and (2) that patients do better when glycosuria and acidosis are removed than when they are allowed to continue."

These are the real issues about which difference of opinion is likely to arise.

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2. Allen. Jour. Amer. Med. Assoc. 1914, 1915, 1916.
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The President asked whether the cases reported were in any way selected as being specially suitable for the treatment; also, how it succeeded in young subjects.

Dr. Walter G. Smith considered that the fact that por-

tions of the pancreas were left in the animal experiments vitiated the results. He had always divided the cases into two groups—the alimentary, which was easily controlled by suitable diet, and the endogenous, which manufactured sugar from body protein and was hopeless. Glycosuria was not really the important thing, but the amount of sugar in the blood. He thought we knew very little as yet of the metabolism of sugar, and that it was often extremely dangerous to meddle.

Dr. Moorhead said that this method of treatment assumed that all cases of the disease were of the same type, which had not been proved, and he thought was not the case. Many cases got on very well on the old lines; and he would hesitate to subject these to such a severe treatment. The three cases in which he had tried it all exhibited marked acidosis. He would be rather afraid of the result in feeble patients, and he would emphasise the point already noted by American authors—that patients who relapse are in a worse position for further treatment.

Dr. Crofton referred to the use of hormonadin and other extracts of pancreas in the treatment of diabetes. He also drew attention to the frequency of focal infections—*e.g.*, pyorrhœa—and urged their appropriate treatment.

Sir John W. Moore disagreed altogether with the principle of this treatment. He objected to starvation on account of the fear of acidosis. A moderate allowance of carbohydrates often staved off a threatening diabetic coma. In addition to general hygienic measures he had much faith in cascara sagrada as a tonic aperient in the costiveness of the disease.

Dr. Thompson related the facts of a case in which he had tried this method. The patient was a Jew, and did well while under observation, but had gone back to his former diet.

Dr. Parsons commented on the extraordinary claim that acidosis disappeared during fasting. Had any explanation been offered? He had under treatment a very severe case, with marked acidosis, and he would like to know whether the method should be tried. If this plan proved successful, we should have to revolutionise our conceptions of the disease.

Dr. Boxwell objected to starvation prior to operation in diabetes on the ground that it would increase acidosis.

Dr. Bewley referred to the possible injurious effect on the nervous system. He also objected to the idea of the patients testing their own urine. They would soon become hypochondriacal or insane.

Dr. Dawson agreed with Dr. Bewley, and thought so much knowledge of their own cases would make patients depressed.

Dr. Peacocke recalled a severe case in which he failed to get the urine sugar-free after three days' fast. Acidosis was present, and he did not persist further.

Dr. Nesbitt, in reply, said that this treatment had been evolved after an enormous amount of accurate scientific work. It was by no means a therapeutic fad. If it proved successful, and the results obtained in large numbers of cases by American observers were remarkable, we would certainly be compelled to revise our knowledge of diabetes. The case he had shown had proved perfectly amenable to treatment. Acidosis had disappeared, as well as sugar. There were no unusual mental symptoms. For the more severe cases some modification of the fat, as recommended by Joslin, would probably be necessary. Among the American cases no selection had been made, and young subjects seemed to do particularly well by this method.

A CASE OF TUBERCULOSIS OF THE GENITAL ORGANS TRANSMITTED FROM HUSBAND TO WIFE.

By Dr. SPENCER SHEILL, F.R.C.P.I.;
Ex-Assistant Master of the Coombe Hospital, Dublin.

[Read in the Section of Obstetrics, November 10, 1916.]

In submitting details of this somewhat unusual and interesting case, I wish to say that portion of the information was supplied to me by a medical friend in the country who prefers to remain anonymous. His care and skill are well known, and any of his findings may be relied upon as being as accurate as possible.

Mr. A. B.—the gentleman concerned—visited his medical adviser complaining of frequent and painful micturition. He had been discharged from one of the Services with a diagnosis of tubercular disease of the bladder. A consultation with an expert surgeon was held, when the right kidney was found to be enlarged and tender, and deposits —probably tubercular in origin—were found in the right seminal vesicle and epididymis. A little later, in order to confirm the diagnosis, tuberculin was injected. The reaction was positive—the temperature rising to 102° F. His kidney was removed very shortly afterwards, and it proved to be very markedly tubercular.

Mrs. A. B. consulted the doctor at the same time as her husband made his first visit. They were then married just five weeks. Previous to her marriage she enjoyed

excellent health. Her complaint was " a tender and painful swelling in the groin," and examination revealed three large and tender lymphatic glands in that region. Careful examination excluded any syphilitic or gonorrhœal infection.

Per vaginam nothing abnormal was found other than some undue tenderness in the broad ligament and ovary on the same side as the enlarged glands. A von Pirquet test proved positive, and a little later she showed a positive reaction to T. B. injection—her temperature rising to 101° F.

She underwent a course of tuberculin injections during which all signs and symptoms of her disease abated. This is well over a year ago now and she remains apparently cured.

Although I will admit that it could be argued that this patient would have developed tuberculosis of the genitals had she never married, I think the circumstantial evidence to be sufficiently strong to justify the opinion that the disease was transmitted by coitus from husband to wife, a theory not, in my opinion, too far-fetched when one contemplates semen from a known tubercular vesicle, being injected directly into the os of an apparently healthy person.

The case certainly opens up the large question of State interference on the subject of marriage of the tubercular, especially if the site of the disease happens to be the genital organs. In the meantime, there is food for thought on the part of the medical man who might be consulted by a tubercular person contemplating the married state.

[The above is exactly as the paper was read before the Royal Academy of Medicine in Ireland. During the dis-

cussion following, it was pointed out that without proof of *local* reaction in the inguinal glands following tuberculin injection, the case was not proved to be one of genital tuberculosis. I have since then elicited from the doctor concerned that there was "marked local reaction," and the omission of this fact from the paper was due to inadvertence on my part.—S. S.]

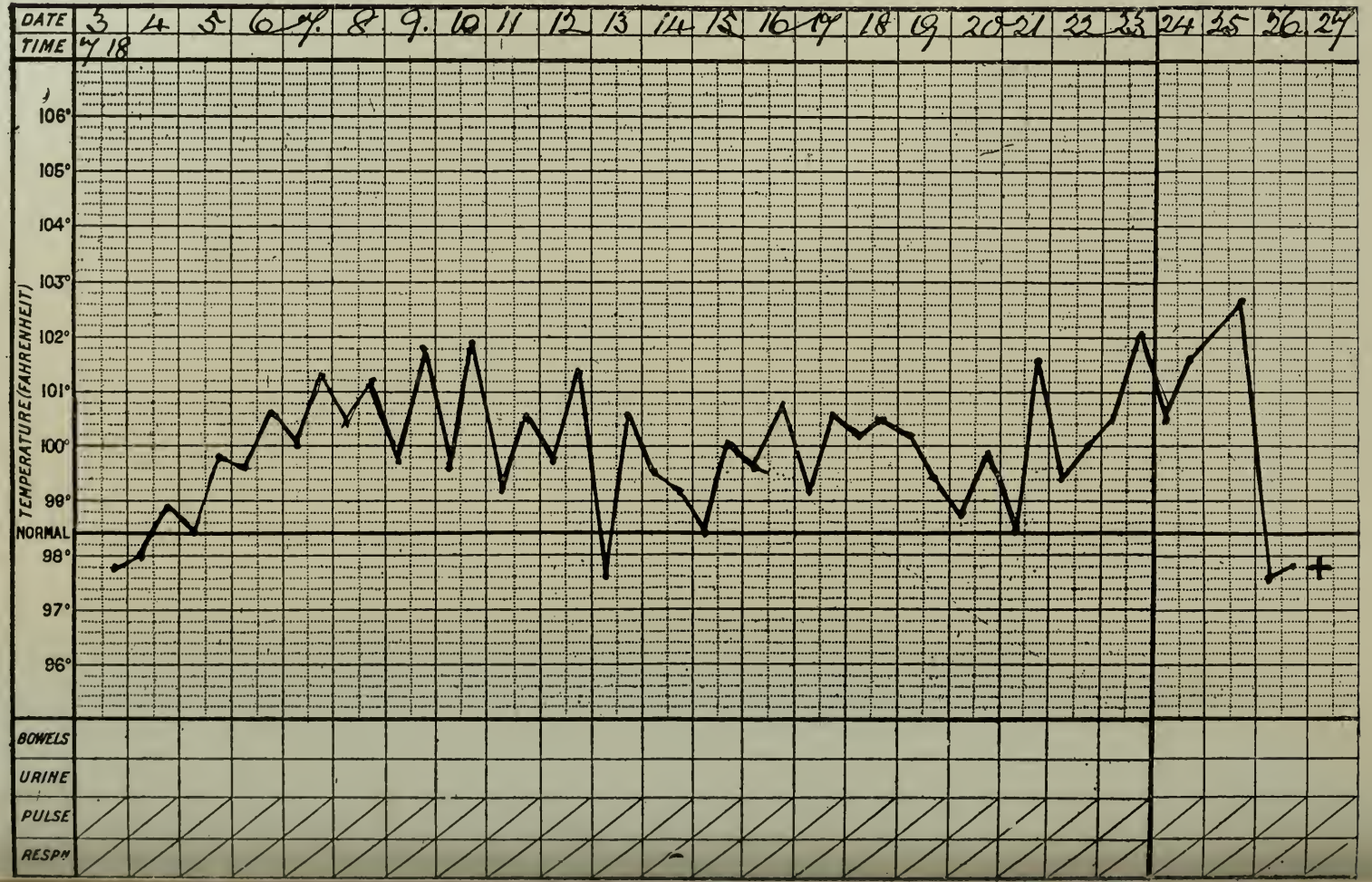
Sir William Smyly said that the case was one of very great interest, and suggestive of the possibilities of direct infection of the wife's genitalia by tubercular semen. He was, however, still unconvinced that the uterus and tubes could be infected in that way. Unlike the respiratory and alimentary organs, the current in the genital canals was in an outward direction, and tended to eliminate the bacilli; nor did it seem possible that they could be carried upwards by spermatozoa. He believed that tuberculosis of the tubes and uterus was never found as a primary disease, but was always secondary to some focus of infection elsewhere, and generally carried to the organs named by the blood.

Dr. Tweedy said tuberculosis required a month or more rest before being active, but it was known that guinea-pigs injected could show tuberculosis in so short a period as three weeks. Tuberculosis was not unknown as a primary ulceration of the cervix; therefore, why should not this occur by coitus?

Dr. Sheill said, in reply, that he did not agree with Sir William Smyly that tuberculosis had no tendency to ascend the female generative organs like the gonococci. There was no more evidence to prove Sir William Smyly's theory than there was evidence to prove that tuberculosis could not be communicated by coitus.

September, 1916.

CHART 1.—*Private A. E. B., aged 29. Pyæmia.*



POST-MORTEM FINDINGS IN TWO CASES OF HECTIC FEVER.

By SIR JOHN MOORE, M.A., M.D., M.Ch., D.P.H. Dubl.; D.Sc. (*Hon. Causâ*), Oxon.; F.R.C.P.I.

Honorary Physician to H. M. the King in Ireland; Senior Physician to the Meath Hospital and County Dublin Infirmary.

[Read in the Section of Pathology, November 17, 1916.]

During the past autumn two cases of hectic or habitual fever came under my observation at the Meath Hospital and County Dublin Infirmary. In neither case was a very accurate diagnosis made during life. But, fortunately, I was able to clear up most doubtful points in the light of the *post-mortem* findings. For the autopsies I am indebted to Dr. R. M. Brontë, the Pathologist to the Hospital. The cases are as follows :—

Pyæmia—Multiple abscesses in many viscera.

Case I.—On September 3, 1916, Private A. E. B., aged 29 years, of B. Squadron, 19th Hussars, was admitted to hospital from the Hospital Ship "Salta." He seemed to be very ill and was much depressed. The Staff Sister of the Military Ward handed me the following memorandum : "Aug. 30, '16.—Pte. B. apparently has a cholecystitis. Two 'Widals' negative. Fæces negative—urinalysis also negative. Leucocytosis, 11,300 on admission. Very tender over left kidney. Temperature 99° to 100° in the morning, and 102° to 104° in the evening. I should like

to hear how he gets on.—C. B. Waite, Capt. C.A.M.C., No. 7 Can. Gen. Hospital, B.E.F., France."

The patient had been many years in the Army, having enlisted at a very youthful age, to which he had apparently added several years when making the necessary statement on enlistment. The family history included at least one case of tuberculosis in a near blood relation—his father died of pulmonary consumption. The personal history pointed to a past alcoholic excess and to an attack of dysentery about seven years ago while he was on active service in South Africa.

On admission Pte. B. weighed only 7st. 11lbs., although he was rather above medium height. He stated that he had lost much weight since his illness began some weeks previously. One day he casually remarked that he had never quite recovered from the kick of a horse in his "stomach" several years ago. On the other hand Mr. George J. MacMunn, M.R.C.S. Eng., the medical attendant of his family at Croydon, Surrey, in a letter of inquiry about him, stated that when he saw him last before he went out to France he was in perfect health, "although he had a tubercular tendency, all his family have it."

At the time of Private B.'s admission to hospital he was fever-free, but his pulse was very quick—110 to 120, and the respiration rate ranged from 24 to 32; the bowels were regular, and the urine was free from albumen and sugar. There was a peculiar bronzing of the skin, but no decided pigmentation about the nipples, or in the armpits. The conjunctivæ did not present a pearly appearance. There were no bluish markings on the tongue or buccal mucous membrane. He had no cough or expectoration.

There was a certain tenderness on pressure in the ap-

pendix region, with dulness on percussion, rigidity of the neighbouring muscles (vital ankylosis), and a fulness. Suspecting mischief in or near the appendix, I asked my colleague, Mr. William Taylor, to see the patient, but he came to the conclusion after a day or two that no operation was indicated for appendicitis or other lesion.

The temperature now resumed the character described in Captain Waite's note—it was septicæmic, with morning remissions and evening exacerbations. The man's appetite was very bad, wasting went on apace, and there was a terrible daily ebbing of strength. Physical examination of the chest revealed extensive congestion of the lungs posteriorly. Dry cupping to relieve the pulmonary stasis brought out intensely dark markings. Also numerous petechiæ developed on the arms and wrists. The urine now contained a mere trace of albumen, but was normal in other respects. Dr. Boxwell examined the blood on September 16th, and reported that the red cells numbered 3,840,000 per cubic millimetre, the white cells were 20,000 per c.mm.; and that there was a decided polymorphonuclear leucocytosis.

Although the patient had neither cough nor expectoration I could not, in view of the family and personal history and in presence of the habitual fever, absolute loss of appetite, and extreme wasting, lose sight of the probability of a generalised tuberculosis. In any case I looked upon him as a dying man, and on conveying this opinion to the Military Authorities, they very kindly arranged that the patient's mother, sister and brother—the last-named a leading seaman in the Royal Navy—should come to Dublin to visit him.

On September 26th the man was evidently dying. Temperature had fallen suddenly during the night from 102.6°

to 97.5°, while the pulse ran up to 140. In the evening of the 26th temperature rose only to 97.8°, and the pulse was 144. He died on the early morning of the 27th.

It seemed to me most desirable to clear up the diagnosis in this case by a *post-mortem* examination, and to this the relatives consented. The case proved to be one of pyæmia.

Dr. Brontë examined the body about 12 hours after death.

The heart showed a slight amount of fatty degeneration. The left pleura had been the seat of a chronic inflammation, leaving some adhesions, which, however, were not extensive. The anterior portions of both lungs were extremely emphysematous; their posterior portions were congested and œdematous—small multiple abscesses were found in both lungs.

The left kidney contained a large depôt of pus, which did not communicate with the renal pelvis. In the right kidney there were a few small pyæmic abscesses. The liver and spleen were enlarged but otherwise normal. The portal blood was sterile.

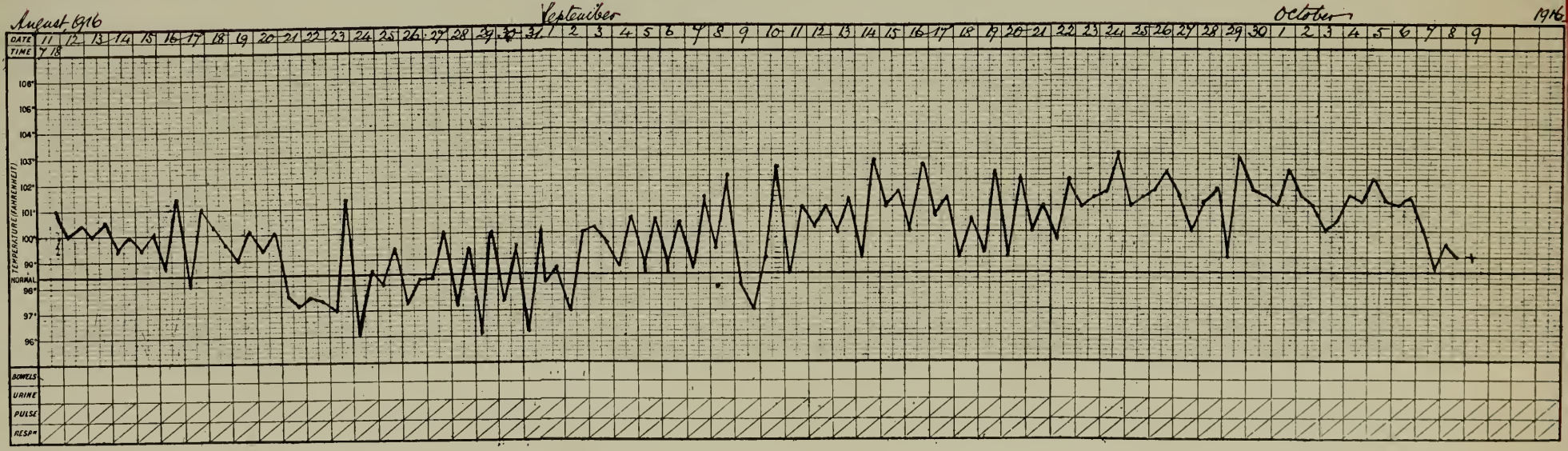
The vermiform appendix was gangrenous. It had been the seat of a chronic suppuration, and the pus had escaped into the pelvis.

Cultures from the various abscesses showed only *staphylococci.*

Tubercular Pleuritis: Disseminated Tuberculosis.

Case II.—J. C., a young man aged 22, a bootmaker by trade, was admitted to hospital on August 11, 1916, suffering from fever (101° F.) and pain over the lower part of the back of the chest on both sides. The illness had come on gradually and insidiously. The pulse-rate

CHART II.—*J. C., male, aged 22. Tuberculosis disseminata.*



was 100 to 108, and the respirations were 28 to 30 per minute. The bowels were constipated—a condition which was constitutional with him, but his appetite was fair. A coated tongue, with red tip and edges, and a somewhat enlarged spleen, suggested enteric fever. There were no rose-spots, and the temperature remained only moderately high—99° to 101.5°. The urine was clear, pale, acid in reaction. Its specific gravity was low, 1010; it was free from albumen and sugar.

Physical examination led to a diagnosis of right pleuritis with commencing effusion. On August 15th, evidence was forthcoming of a considerable effusion into the right pleura and a moderate effusion into the left pleura. There was no œdema over the right side of the chest, but the occurrence of rigors and the temperature-ranges suggested an empyema. I asked my colleague, Dr. Boxwell, to see the patient with the view of exploring the pleura, but he advised against tapping it as the amount of effusion was not causing distressing symptoms. At no time had the patient any cough or expectoration, and so we were debarred from making a bacteriological examination.

Meanwhile the heart was daily becoming weaker and its beat more frequent. Wasting set in and a general failure of strength, accompanied by an increasing tendency to tremor on any muscular movement. On August 21st a further complication appeared. Intense pain in the right foot, most intense in the little toe, was complained of, interfering with sleep. The foot was pale and cold to the touch, and the little toe was already discoloured, as if local asphyxia was present. In a day or two it was quite clear that the condition was one of dry gangrene. At Mr. William Taylor's suggestion, the toe was lightly painted with tincture of iodine, in addition to friction and envelop-

ment in wadding—means which had already been adopted to combat this serious complication.

On September 1st, a distinct pleural friction sound was audible over the base of the right lung; the percussion note was dull and the breath-sounds were diminished. The continuance and the character of the pyrexial temperature suggested the presence of an acute miliary tuberculosis, but there was no sputum, so that it was not possible to verify this view of the case—so far as the lungs were concerned—by a bacteriological test.

The leading features in the case during September were increasing weakness and tremor, progressive loss of flesh, and the occurrence of epileptoid attacks at intervals of several days. For the last complication, as I may call it, a modification of Brown-Séquard's compound bromide mixture for epilepsy was prescribed and taken with benefit for some time. For constipation also a cascara sagrada mixture with tincture of nux vomica was ordered.

At the beginning of October the patient was failing fast. His breathing became laboured, and fine and coarse crepitating râles developed all over the chest, particularly at the back. The patient grew more and more drowsy, and finally lapsed into unconsciousness. Death supervened on the early morning of Monday, October 9th.

The *post-mortem* examination was made by Dr. Brontë, who kindly gave me the following brief summary of the findings :—

Acute pleurisy on both sides; disseminated tuberculosis of both lungs; the left lobe of liver overlapped, and was adherent to, the spleen; the latter organ showed a large area of caseation. There was no enlargement of the mesenteric or mediastinal glands. The diaphragm and pericardium were studded with tubercles.

A glance at the temperature charts in the two brief clinical records, which I submit to the Section of Pathology, will show that they resemble one another in the irregularity of the temperature ranges and the persistence of a febrile condition—well-known characters of habitual or hectic fever, synonymous terms derived from Latin and Greek respectively. In each patient this characteristic type of pyrexia was caused by a specific infection—pyogenic in the first case, tubercular in the second case.

When Private B. was admitted to hospital, I came to the conclusion, after careful investigation, that he was suffering from appendicitis. That was the reason why I called Mr. Taylor into consultation. When the localising symptoms and signs subsided, and on learning more about the patient's family history, I frankly admit that I changed my opinion and thought that a disseminated tuberculosis more likely was present. The *post-mortem* examination proved that the former diagnosis was much nearer the truth. I may mention that the relatives were much relieved, even gratified, to learn that the autopsy lifted the incubus of a familial tubercular tendency which had been weighing heavily on their minds. Here, then, is a practical illustration of one of the uses of a pathological investigation into the cause of death.

In the second case the *post-mortem* findings merely confirmed the diagnosis of a disseminated tuberculosis which had been made from the history and progress of the case. The absence of cough and expectoration until the final stage of an illness extending over many weeks is noteworthy.

The Chairman laid stress upon the importance of bringing the full clinical details as well as the *post-mortem* findings before the Section. The lesson learnt by having all the

aspects of a case were useful in the elucidation of future cases. In the first case he regarded the abscess in the kidney region as being the primary cause of the pyæmia. He had had experience of several cases of pyæmia and septicæmia where the origin of the infection was obscure. He had no doubt that in some cases the site of entry was the tonsil, or that infection sometimes arose through extreme conditions of pyorhœa alveolaris.

Dr. T. T. O'Farrell gave the details of a somewhat similar case of cryptogenetic septicæmia, which failed to be influenced by ordinary therapeutic measures. Blood cultures proved sterile, and the serum did not agglutinate either *B. typhosus*, *B. para-typhosus* A. or B. Cultures made from the throat revealed a pure culture of streptococcus. The administration of a vaccine, made from this micro-organism, was followed by an almost immediate amelioration of symptoms, and an apparent cure.

Dr. W. M. Crofton suggested that a bacteriological examination of the urine might prove of use in obscure cases, even when an examination of the centrifugalised sediment failed to reveal any gross pathological condition. He considered that the glomeruli played an important part in allowing the exit of micro-organisms. With regard to the second case, he had found the subcutaneous tuberculin test of great value in diagnosis, but considered that much smaller doses than were usually given were quite sufficient to elicit a diagnostic reaction.

Sir John Moore, in reply, said that he regarded the kidney lesion in the first case was the primary cause of the pyæmia. He said that he had omitted to mention that there had been a previous history of injury in the region of the kidney.

THE "LATENT PERIOD" IN ACUTE PERFORATIONS OF THE STOMACH OR DUODENUM.

By DENIS KENNEDY, F.R.C.S.I.;

Surgeon to St. Vincent's Hospital, Dublin;
Surgeon to the Children's Hospital, Dublin, &c.

[Read in the Section of Surgery, November 24, 1916.]

Although a great deal has been written about acute perforation of the stomach and duodenum, still the early diagnosis of the calamity is frequently missed. Not alone is an accurate diagnosis not made, but the fact that a grave intraperitoneal lesion has occurred is completely overlooked. This, I believe, is largely due to the fact that, contrary to what one would expect from reading the text-books on the subject, the symptoms in this condition are not progressive and the physical signs are not developed until comparatively late. In other words, two conditions arise—first, the perforation, and secondly, the development of peritonitis; between these two a varying interval may elapse which may well be called the "latent period." To illustrate this I cannot do better than give a short history, from many, of a couple of cases that recently occurred in my practice.

Case 1.—A short time ago I was called to see a case, in consultation, about 50 miles from Dublin. I arrived there a couple of hours after receiving the message, on a Tuesday afternoon. The patient was a lady about 50 years of age, who had enjoyed good health, unless for an occasional attack

of dyspepsia, up to a couple of days previously. On the previous Sunday night she had a hearty supper at about 9 o'clock. She felt uncomfortable after this, but went to bed as usual and slept until about 4 a.m., when she awoke with violent pain in the abdomen, vomited, and felt intensely weak. Her doctor saw her at 9 o'clock on Monday morning. She then described herself as having some abdominal pain, or more discomfort than pain, but she was gradually improving, and felt very much better than she had some hours previously. The patient and doctor were inclined to attribute the trouble to the supper taken the night previously, and the condition was not then regarded as very serious. All day on Monday her condition remained pretty much the same. On Monday night she got a $\frac{1}{4}$ grain of morphia hypodermically to produce sleep; this had the desired effect. On Tuesday morning, when the doctor again saw his patient, he got an unpleasant surprise. She looked badly, had a quick weak pulse, and her abdomen was markedly distended. He at once recognised that acute peritonitis was rapidly developing, and sent for me with a view of having operation carried out.

When I saw the patient early in the afternoon she was dying. All the symptoms and signs of diffuse peritonitis were then present. She lay on the back motionless, with the legs well drawn up; her breathing was altogether thoracic in character; her general aspect could not be worse. Her pulse was almost imperceptible and could not be counted, and her temperature was subnormal. She did not complain of pain, and her mind was perfectly clear. Her abdomen was distended to almost bursting-point; she begged for something to be done for her. While I was discussing with her doctor the futility of operating the patient died.

Both of us were quite agreed that the cause of the fatality was gastric or duodenal perforation, probably the latter, on account of the comparative absence of symptoms previous to the attack. The early diagnosis of the condition was entirely missed by as able a practitioner as there

is in the provinces. He was completely misled by the condition of the patient when he first saw her, by her persistent statement that she was rapidly improving, by the complete absence of well-marked physical signs, and by the fact that she remained thus during the whole of his first 12 hours' attendance.

The second case occurred within a week afterwards, but fortunately had a very different ending.

Case 2.—Miss ——, aged 22 years, was admitted to St. Vincent's Hospital on a Sunday afternoon about 5 o'clock. She was examined by the house surgeon, Dr. A. Courtney, who promptly diagnosed gastric perforation, and sent for me, while at the same time he made preparations to have an operation carried out. Being Sunday I had some distance to get back to Dublin after receiving the message, but I saw her at 8 p.m., and got the following history. The patient had suffered from repeated stomach trouble for some years and had received a good deal of medical treatment for the condition. She retired to bed on Saturday night in her usual health, but awoke on Sunday morning about 5 o'clock with violent abdominal pain. She vomited once and then almost fainted. After a couple of hours her symptoms practically passed off, and she did not send for a doctor until 4 o'clock in the afternoon. She drank fluids pretty freely during the day. The doctor sent her unlabelled to St. Vincent's Hospital. On examination, about 16 hours after the attack beginning, her appearance was by no means bad; she did not complain of much pain, but felt decidedly uncomfortable. Superficially one would not think that she suffered from any grave abdominal lesion. She lay rather naturally with her legs fully extended; her breathing was by no means distressed and was not wholly thoracic; her pulse-rate was, however, suggestive, being from 110 to 120; her temperature was normal. Her abdomen was somewhat distended, and although tenderness was present, it was by no means well marked, and was principally above the umbilicus.

There was almost a complete absence of rigidity of the abdominal muscles. Although one might miss the diagnosis, I had no hesitation in confirming Dr. Courtney's opinion.

Operation.—On opening the abdomen by incision to the right of the mid-line I found a large quantity of a greyish-green fluid everywhere present in the peritoneal cavity. I did not notice any free gas present. The visceral peritoneum was everywhere injected, I saw no pus and no sign of lymph formation anywhere; the intestines were generally distended but not to a great extent. After some time I located a perforation, into which I could put the tip of the index finger, at the extreme cardiac end of the stomach. To reach the perforation I had to prolong the incision right up to the ensiform cartilage. To close the perforation in this situation was by no means easy. By getting Dr. Courtney, who was assisting, to pull the liver upwards with one hand, while with the other he drew the stomach downwards, I was enabled to bring the site of the perforation better into view. I attempted to pass through-and-through sutures for closure, but they at once cut their way out. This was due to the large area of ulcerated and inflamed tissue surrounding the perforation. Consequently I had to close the perforation by sero-muscular sutures. These were interrupted in character and extended from well above the perforation to well below it. They were outside the ulcerated area, and by their insertion the ulcer was completely inverted and large surfaces of peritoneum were brought into apposition. I inserted only one row of these and no other means of closure was adopted. The cardiac end of the stomach was considerably narrowed in lumen when the sutures were tied, but no ill-effects seemed to follow. I removed the contents of the stomach that had escaped into the peritoneal cavity by gently swabbing with dry sponges. I used no douche, as is my invariable practice in all cases of peritoneal infection. Closure of the wound and providing for free drainage completed the operation. I placed a drainage-tube with gauze wick (1) between the liver and the diaphragm; (2) above the pubes; (3) one in each flank. These latter three I inserted by stab incisions, easily

made when the abdominal wall is made to project from the inside on the finger or a forceps. During the operation and subsequently before she regained consciousness the patient received about 8 pints of normal saline solution administered under the breasts. The recovery was rapid and uneventful. No pus formation occurred after the operation.

The absence of pus or of purulent peritonitis either at the operation or subsequently is probably due to two or three causes. 1. The stomach was empty of food when the perforation occurred, consequently its contents were not highly septic. 2. The fact that the perforation occurred high up. I think we may take it as pretty certain that the more we descend in the gastro-intestinal canal the more septic the contents become. 3. The free drainage provided for at the operation. I am indebted to Dr. Courtney in this case for his prompt diagnosis, his able assistance at operation, and his after-care of the patient. I would emphasise the fact that when I saw her first, 16 hours after the perforation occurring, the symptoms were not severe and the signs of diffuse peritonitis were not at all very pronounced.

In both these cases perforation occurred when the patient was asleep and when the stomach must have been free from food. This would point to the conclusion that auto-digestion plays a great part in the occurrence. To look at these cases of perforation from a proper standpoint we must remember that an interval varying in length elapses between the two acts of the tragedy—first the perforation, then peritonitis ; and that for treatment to be successful our interference must take place before the second act is staged, or, at all events, before it is long in existence. Consequently early diagnosis is essential.

To simplify early diagnosis I consider it convenient to divide the course of events as regards symptoms and signs

into three stages: (1) The stage of shock and collapse, (2) the stage of reaction, (3) the stage of diffuse peritonitis. One need only remember the sequence of events to see why each of these set of symptoms is brought about. The shock as well as the pain is altogether due to the injury which the peritoneum receives from contact with the stomach or intestinal contents. When the stomach is emptied, as it pretty quickly will be, by extravasation of its contents and by vomiting, the injury to the peritoneum ceases, until septic conditions develop. Unless the patient has received a knock-out blow reaction sets in, and the patient may be comparatively well sometimes for several hours. The length of time will depend on the degree of septicity of the contents escaped into the peritoneal cavity and on the amount. The development of peritonitis requires no explanation.

It is frequently during this time of reaction or latent period that the medical attendant first sees the patient after the attack has begun. Consequently his difficulty of diagnosis. He finds little on examination; the patient may look well, and usually asserts that improvement in his or her condition is decided. Symptoms are more or less in abeyance and physical signs are not yet well marked, and if the practitioner's knowledge of acute perforations has been derived from books and not from experience there is risk that the diagnosis will be missed. How, then, is the diagnosis to be made? The history of the case previous to the attack is of some value; the history of the attack itself is absolutely essential.

An acute attack of violent abdominal pain associated with severe shock and collapse, and followed by an improvement in the mind of the patient, is always significant. Add to this an increasing pulse-rate with a sub-

normal temperature and there is quite sufficient to warrant operative interference. Vomiting is not of much significance, as it may be absent in perforations, and it is present in so many other conditions.

Of course, there will be physical signs elicited on a careful abdominal examination, but not so pronounced, as is usually believed, previous to the development of peritonitis. Great stress is laid on the rigidity of the abdominal wall. True, there will be rigidity present, but at this stage of the trouble it will be strictly confined to the area of the abdomen involved, and I have never seen it so marked that it could be described, as it often is described, as being " board-like " in character. Tenderness will also be confined to the region of the perforation, and no decomposition will have occurred either in the peritoneal cavity or in the abdomen to produce marked distension. So that during the " latent period " the diagnosis will have to be made (1) from the history of the attack ; (2) an increasing pulse-rate without corresponding increase in temperature ; (3) localised rigidity of the abdominal muscles ; (4) localised abdominal tenderness ; (5) beginning abdominal distension.

Fortunately, when the diagnosis is made there is no difference of opinion as to the essentials of treatment. These are altogether operative, with two main objects in view : to repair the lesion and to get rid of the foreign material from the peritoneal cavity. I always get rid of this material by dry sponging, gently used. I never douche, as I believe it does more harm than good, especially by bringing the infective material to the most remote corners of the abdominal cavity. Besides it takes time. I usually employ drainage, always when the soiling of peritoneum has been present for a few hours.

There is one other point about which a legitimate differ-

ence of opinion exists in the minds of surgeons. Should gastro-enterostomy be performed at the same time if there is indication that this procedure will be required later? Personally I am decidedly of opinion no, if it can be at all postponed. The further time required may turn the scale completely against the patient; besides, we are working in an infected area, and this is not conducive to good results. But in the last few weeks I had a patient from Mr. T. M. McEvoy, of Blackrock, with stomach perforation, in which I had to perform gastro-gastrostomy, and with the happiest results.

This patient had a perforation in the centre of a large ulcer on the anterior wall of the stomach about the junction of the upper and middle thirds. When I had the ulcer inverted the stomach was divided into chambers with only a small opening between. I exposed the posterior wall in the usual manner through a rent in the transverse mesocolon. I did an anastomosis by suture between the upper and lower pouches. The patient made a complete and rapid recovery and was apparently free from all stomach trouble when leaving the hospital three weeks after operation.

I owe an apology for bringing up for discussion a subject about which so much has been written, but I hope to have done something to direct attention to the early and accurate diagnosis of a condition which, when not detected promptly, is nearly invariably fatal.

EXCISION OF THE ELBOW FOR GUNSHOT INJURIES.

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[Read in the Section of Surgery, November 24, 1916.]

Having had the opportunity while working in France during the last two years of both performing the operation of excision of the elbow for gunshot injuries and of seeing the results of operation at periods varying up to eighteen months, I venture to bring this subject before you for consideration.

One of my earliest hospital recollections is seeing a man who had had his left elbow excised by the late Mr. H. G. Croly shown to the class at the Royal City of Dublin Hospital. He was a sailor, and had continued his occupation since the operation, which had been performed some three or four years previously. He had movement of the elbow through an angle of about 140 degrees, and the muscular power was sufficient to flex or extend the joint against the force of any one of the students present. Even at the present day this would be considered as a very good result, and it must be remembered that this operation had been performed in the very early days of antiseptic surgery. The following is a brief history of the cases under my care :—

Case I.—A Zouave was shot on September 20, 1914. The missile, probably a deformed rifle bullet, struck the left

elbow joint, smashing the bones extensively. When first seen by me in December he was discharging from several sinuses on the posterior aspect of the joint. These healed up under treatment. An *x*-ray photograph showed osseous ankylosis of the joint at an obtuse angle, with great deformity of the lower end of the humerus and upper ends of the radius and ulna, with numerous small fragments of metal embedded in the bones. Operation was performed on January 15, 1915. About one inch of the humerus was removed, and the olecranon was prized off and the upper end of the ulna trimmed; no attempt was made to remove the ends of the bones of the forearm along the normal saw cuts, owing to the great destruction and irregular formation of the bone ends and the fear of opening up quiescent septic foci in the neighbourhood of the embedded pieces of metal. A very slight infection occurred in this case, but it rapidly cleared up, and the wound healed completely. At first we had some difficulty in getting the patient to move his arm, and he complained of pain over the upper end of the ulna which projected under the skin, so on March 24 the projecting piece of the bone was chiselled off, and the opportunity was taken at the same time to forcibly move the joint. After this regular massage and electrical treatment were undertaken, and the range of voluntary movement was rapidly increased; the patient left the hospital in about six weeks, with voluntary movement of about 110 degrees. When I returned to France last summer I had an opportunity of seeing this patient again. He had been discharged from the Army, and was working in a munition factory. He could use his left arm for his work, and could flex and extend his elbow against very considerable resistance. The range of movement was from about 40 degrees of flexion to 160 degrees of extension. There was only slight power of pronation and supination, the radius and ulna moving together as there was bony union between their upper extremities.

Case II. was a soldier who had been wounded in September, 1914, by a shrapnel ball which had entered the left elbow from behind through the olecranon, and was lodged in the soft tissues in front of the joint. A septic arthritis had followed, and in March, 1915, the joint was ankylosed at a right angle, and a suppurating sinus still existed. On March 5, 1915, my colleague, Dr. Meade, excised the elbow, removing the humerus above the condyles and also the olecranon and top of the coronoid; the head of the radius was not touched. The cavity was douched with carbolic lotion and packed with iodoform gauze, and sewn up in the usual way. In spite of transient infection the wound healed rapidly and soundly. Within a month of the operation this patient had very fair voluntary movement of his joint.

Case III.—Soldier wounded, March 16, 1916, by shell, there was osseous ankylosis of the right elbow at an angle of about 130 degrees. The *x*-rays showed an old fracture of the olecranon and the formation of callus in front of and behind the lower end of the humerus; pronation and supination were completely lost as well as flexion and extension. Operation was performed on the 22nd of July, 1916. The olecranon and condyles were cleared and the olecranon was separated from the humerus by the chisel; the lower end of the humerus was dislocated through the wound and sawn off above the condyles, and the corners were rounded off. There was some difficulty in clearing the ulna and radius, owing to callus formed in the anterior ligament, the articular bearing portions of both bones were removed at the level of the lower border of the head of the radius, after which pronation and supination became possible. This case healed without infection, and massage and electrical treatment were commenced within ten days of the operation, and by the end of a month the patient was able to lift his hand to his mouth. When I left the hospital in the beginning of

September he had voluntary movement through an angle of about 130 degrees, and was already able to use his arm for feeding himself and writing. There was limited movement of pronation and supination.

Case IV.—Soldier wounded February 25, 1916, by a grenade in the left elbow. There was osseous ankylosis of elbow with irregularity of the articular surface of humerus, and fragments and dust of the grenade were scattered through the bones and soft tissues; all the sinuses which had been present were healed. Operation was performed on August 31, the bones were cleared, and the radius and ulna separated from the humerus by the chisel. The end of the humerus was sawn off just above the condyles, the corners were rounded off, and the head of the radius and the articular surface of the ulna were removed, leaving the orbicular ligament and its attachments intact. In this case infection occurred and necessitated frequent dressings, syringing with Dakin's solution and drainage. The last account I heard, however, nearly a month after the operation, was that the wound was almost healed and there was already fair voluntary movement. This man had been operated on, in a hospital close to the front, to fix the elbow in this position as being the most useful one for a farm labourer, which was his occupation.

In older war surgery an important distinction was drawn between primary and secondary excision of the elbow, the results of primary excision being much the better. The late Sir William McCormac, in his "Notes and Recollections of an Ambulance Surgeon," which he published after the Franco-German War, gives as his results one death in four cases of primary excision, whereas there were 5 deaths in 7 secondary operations. In the present day I imagine that a typical primary excision is never done, the practice adopted being to wait and see

whether an excision will become necessary after healing has occurred. The nearest attempt to a primary excision that came under my notice was the following :—On March 10, 1915, I was called to see a soldier who had been wounded in the left elbow by a shrapnel bullet on February 28, 10 days previously. There was a large suppurating wound over the external condyle of the left humerus. The wound was enlarged and explored, and over a dozen pieces of bone, some nearly half-an-inch in size, were removed; the external epicondyle was broken, but still attached by muscles; the internal condyle, head of radius and olecranon were apparently uninjured. The wound was swabbed with carbolic and iodine, and packed with iodoform gauze. This case did well and recovered with a moveable joint. But it will be seen that it was only a very partial excision, and was not performed till 10 days after the injury.

In civilian practice the chief indications for excision of the elbow are :—I., ankylosis, fibrous or osseous, resulting from injuries, usually fractures of the bones entering into the formation of the joint, the most frequent being fractures of the lower end of the humerus; and II., cases of tubercular disease of the joint. It may occasionally be required in III., cases where ankylosis has resulted from a septic infection of the joint, but these cases are rare. In civil practice, therefore, it is unusual for sepsis to be present, and with ordinary precautions there is no reason to fear infection after the operation, the wound should heal without suppuration. In military surgery the conditions, however, are very different. Here the ankylosis of the joint resulting from gunshot injuries is nearly always accompanied, if not actually caused, by sepsis, and even if the operation is not performed until

after the tissues have apparently soundly healed, there is always a grave danger that during the operation some encapsuled and quiescent septic focus will be opened up, and the large cavity in the tissues left by the excision of the joint will become infected and lead to prolonged suppuration with its attendant formation of cicatricial tissues.

During my stay in France, I gathered that the results of this operation among the French were so far from satisfactory, that in cases of fibrous ankylosis every effort was made to obtain even a slight degree of movement by prolonged treatment with mechanotherapy, and in cases of osseous ankylosis it was recommended to make a linear section of the bones and put the arm up in the position most useful for the particular occupation followed by the individual. For the left arm of a labourer—for instance, as in Case IV.—fixation at an angle of 130 degrees was recommended. This certainly cannot be looked upon as a counsel of perfection and with less than this the medical profession should not be satisfied.

I am firmly convinced that excision of the joint is the proper treatment for all cases of osseous ankylosis and the majority of those of fibrous ankylosis, at least those where it is unlikely that 90 degrees or more of movement will be obtained, or where this result has not already been attained after a reasonable trial of mechanotherapeutic measures.

The operation must, however, be performed by a competent surgeon who has had some experience of this particular operation, and certain precautions must be taken.

Of the various incisions which have been suggested for this operation, by far the best is the vertical posterior incision of Pott. In this connection I was interested in

coming across the following passage in the late Sir William McCormac's "Notes and Recollections of an Ambulance Surgeon." In describing a case which had been admitted to the Anglo-American Ambulance, he says :—"The joint had been cut out through a transverse incision, across its posterior aspect; there was no attempt at repair, and the wound was a large hole, into which one might almost put a closed fist. The forearm was only attached to the arm by a narrow isthmus of skin and muscle in fact; we received some other cases of resection of the elbow which had been performed elsewhere in this manner—that is, by a transverse in place of a longitudinal incision. The men told us that they had been operated upon by German surgeons, but the method adopted is one which would find little favour in this country."

Again, he says :—"The operator had cut transversely across the back of the elbow from one side of the limb to the other, sacrificing the ulnar nerve, of course, and making a huge, unsightly wound."

The incision is carried through all the tissues down to the bone, splitting the triceps, muscle and tendon in the upper half, and the deep fascia and periosteum covering the olecranon and posterior border of the ulna in the lower half. This is the most important step in the operation, and the one where most mistakes are made. In order to obtain a useful arm afterwards it is absolutely necessary to maintain the attachment of the triceps to the deep fascia on the back of the forearm, and unless great care is taken in raising the periosteum from the back of the olecranon there is grave danger of this continuity of tissue being lost. This is the only part of the operation that need be done subperiosteally. The other steps of the operation are comparatively simple, the tissues on either side of the

incision are separated from the bones, special care being taken on the inner side to avoid injury to the ulnar nerve; the bones of the forearm are then separated from the humerus by division of the lateral ligaments and fibrous tissue in cases of fibrous ankylosis and by the chisel if there is bony union, and the ends of the bones are dislocated out of the wound, and sawn off. As little as possible of the bones of the forearm should be sacrificed, the bicipital tuberosity of the radius and the coronoid process of the ulna being preserved at all costs; in ideal cases only the portions of the bone covered by cartilage need be sawn off. It matters little how much of the humerus is removed, the danger of removing too little is greater than the danger of removing too much. The saw cut should always pass above the epicondyles; and probably a good result might be obtained even in a case where the humerus had to be sacrificed in its lower fourth. The danger of removing too little is that ankylosis may persist unless an arthroplasty according to Murphy's method is performed instead of a simple excision. It might here be suggested that an arthroplasty was the preferable operation; its disadvantages are that it entails further dissection and separation of the tissues which is most undesirable in these cases, which, as explained, are liable to infection; owing to this liability to infection, there is also a danger of sloughing of the flap of fibrous or fatty tissue which is inserted into the joint between the ends of the bones; again, this flap has to be introduced into the joint between the edges of the divided triceps, and may, therefore, interfere with their proper union and the later recovery of function by the triceps muscle. Finally, if the operation of simple excision is correctly performed a perfect functional result may be obtained.

The operation is concluded by ligature of all bleeding joints, considerable oozing will always persist both from the cut ends of the bones and the divided tissues, and this is best controlled by tight packing of the cavity with iodoform gauze. The two edges of the triceps and its continuation into the periosteum and fascia of the forearm are united by a continuous suture of catgut, except for a small opening left for the emergence of the gauze; this should preferably be placed at the upper end in the muscular portion of the incision, so as to insure good union of the periosteum and fascia with its poorer blood supply. The skin may be closed by Michel's clips or silk-worm gut sutures.

Usually the gauze packing may be left for 48 hours before removal, by which time liability to continuation of the oozing has ceased, the edges of the wound may be kept separated for a few days longer by a small gauze mesh, and the cavity gradually obliterated by pressure of a voluminous dressing. In cases where sepsis supervenes it will be necessary to use a rubber drain and wash out the joint cavity with eusol or other lotion daily, but even here complete closure can usually be obtained within a month or five weeks.

In my experience no splint is required in the treatment of these cases, the joint is surrounded with a thick layer of wool extending high up on the arm and low down on the forearm, and held in position by a tightly applied bandage. Sir William McCormac says:—"Unless for purposes of transport, it is not often necessary nor even, I consider, desirable to apply splints of any kinds to excisions of the elbow joint. The after purpose of the operation is to secure free motion in the new articulation, and the arm and elbow can usually be adequately and well supported

by pillows. I myself never used any form of splint after excision of the elbow, and I found that great comfort to the patient and great facility in applying new dressings were thereby secured."

An essential point in the after-treatment is massage and electricity, which should both be adopted at an early stage to obtain a return of the function of the triceps and biceps which are paralysed from long want of use. As seen by the history of Cases II. and III. in favourable circumstances good voluntary movement may be obtained within a month after operation; and after an interval of a year or so the patient's arm may be to all intents and purposes as strong and as useful as ever.

TREATMENT OF RHEUMATIC CONDITIONS.

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Rheumatic conditions, like many others, appear to be septicæmias, with focal manifestations. In typhoid fever, for instance, the focal manifestations are in the intestine; in pneumonia, in the lungs; in rheumatic infections, in the joints, muscles, or nerves, while other organs, such as the heart and kidneys, may be involved.

The clinical conditions fall naturally into two groups—the acute and chronic; and this classification is suitable pathologically, for the chronic cases, unless sometimes when they are a sequel to the acute, are usually complicated by secondary infections, while the acute conditions are generally produced by a single infection. They all appear to agree in having a primary focus of infection, from which the blood and other organs become infected. Acute non-suppurative inflammations of joints occur in rheumatic fever, gonorrhœal infections, and acute gout. Acute gout will be considered with the chronic variety, acute gonorrhœal arthritis need not be considered here.

In rheumatic fever the most common primary focus of infection is in the tonsils, adenoids or gums; from these the blood stream becomes infected *viâ* the lymphatics.

The onset of the fever is accompanied by lesions in the joints. Where there is considerable inflammatory infiltration of the subsynovial and periarticular tissues, the cartilages are swollen, their cells proliferated, and there is more or less effusion into the joints. Lesions in other tissues may follow—endocarditis, which is the rule rather than the exception; myocarditis, pericarditis, pleurisy, pneumonia, &c.

Diagnosis.—The causal organism has been studied by many people, notably Poynton and Payne and Beattie, and has received various names—*e.g.*, the *Diplococcus rheumaticus*. It is morphologically and culturally identical with the streptococcus commonly found in pyorrhœa alveolaris, tonsils, adenoids, &c. It can be isolated from the primary lesion, from the joints, from the blood, and it will most probably be found in the urine and fæces in these cases. As in the case of all other microbes, its virulence varies.

Prophylaxis.—Since there is nearly always a primary lesion in the regions already indicated, and this lesion is usually present for a longer or shorter time before the symptoms of acute general infection arise, it is obvious that the clearing up of such training grounds for pathogenic microbes will prevent the onset of the general infection. Tonsils and adenoids should be removed, the streptococcus isolated from them or from the pus from the gum, if that is the lesion, and a course of vaccine given. The sequence of doses, $2\frac{1}{2}$, 5, $7\frac{1}{2}$, 10, 15, 20, 30, 50, 75, 100 million, will usually be found suitable and adequate. Care must be taken that the patient should not be overfatigued or exposed to chill for twenty-four hours after a dose. The intervals must be regulated by the occurrence of reactions. No dose must be given with a shorter

interval than 48 hours. If reactions, focal or general, occur, the next dose must not to be given until three days after all signs of reaction have disappeared.

Treatment.—I think I may fairly state that the sole methods of treatment at present in use are rest and salicylates. As to the adequacy of these measures, I will quote an article by French in the *Medical Annual* for 1916. He says :—" The fact that salicylates in rheumatic fever do practically nothing towards curing the disease is apt to be lost sight of. What salicylates do is to bring down the temperature and relieve the joint pains, an immense boon to the patient, but the pains will recur if the salicylates are stopped, up to a period which is approximately the same as used to be the case before salicylates were employed. . . . It is, however, probable that the mere fact that salicylates relieve the joint pains renders the severity of the subsequent valvular heart disease even greater than before; for endocarditis is not prevented by salicylates, and some patients who would rest in bed longer if they were in pain, get up, and add unnecessary strain to their inflamed heart valves; when not being in pain, they believe they are cured of their acute rheumatism."

The universal method of treatment then is entirely inadequate, either to cure the disease or prevent relapse.

My personal opinion is that a combination of chemo- and immunotherapy will give the best results. I know from clinical experience that an iodine salicylic acid compound in the form of the sodium salt of the di-iodosalicylic acid is a powerful intravital killer of streptococci, and I think it would be well worth a trial in acute rheumatism. The doses I would suggest would be 2 to 5 cc. of a 1 per cent. solution given daily intravenously.

As soon as possible an autogenous vaccine should be

made, the dose being 2½, 5, 7½, 10, 15, 20 million, and more if necessary. Even if a vaccine is not used in the acute stage, as I am quite sure it should be, it should be used in order to prevent relapse, which is notoriously common after an attack.

Acidosis—so constant in the disease—should be combated by giving sodium citrate until the urine is amphoteric.

Chronic Rheumatism.—In sub-acute and chronic rheumatism the exudate in the joint and surrounding tissues becomes gradually organised. The cartilages may be at first unaffected, or may be so only near their margins, where they may become eroded. Later these erosions may spread until the whole cartilage is destroyed, and bony ankylosis may result from spread of the infection to the bones (*rheumatoid arthritis*). In other cases the cartilages may be eroded and destroyed where there is pressure, while they become hypertrophied and ossified at the edges, so as to produce osteophytes, some of which may be broken off, and become loose in the joints. Where the bare bones are in contact, they become polished and atrophied. This atrophy may be so extreme that eventually the whole head of the bone disappears (*arthritis deformans*).

In chronic gout sodium urate, or the urates of calcium, magnesium, or ammonium, become deposited in the articular cartilage, which becomes eroded. Later these urates become deposited in the synovial membrane, periarticular tissues, ligaments, tendon sheaths in the periosteum, bursæ, deep connective tissues, and subcutaneous tissue.

Diagnosis.—Chronic rheumatism attacks men and women of all ages, and gout attacks people from middle age onwards, who are usually of an alcoholic habit. Rheu-

matoid arthritis affects people, usually females, between 20 and 40, and arthritis deformans is commonest in females between 40 and 60.

I am perfectly aware that it is the general opinion that each of these clinical conditions—rheumatism, rheumatoid arthritis, arthritis deformans and gout—has a different ætiology. I have reason to think, however, that this is not the case, but that the mechanism of their production is the same, and that the differences in the pathological condition are due to differences in the quality of either the toxins or the tissues attacked by them, and the quantity of urates in the patient's circulation. This opinion has been reached as the result of observing that they all have similar foci of infection other than those of the joints, and as the result of favourable effects on the joint conditions produced by measures adopted to cure these other lesions. For the large majority of all such patients have chronic infections in the upper alimentary or respiratory tract, the microbes from which can and do infect the rest of the gastro-intestinal tract, which is evident from signs and symptoms. These microbes produce the primary infection, and lower the resistance of the tract; the coliform microbes inhabiting the intestine then become pathogenic, so that there is a mixed infection. This chronic inflammation not only interferes with normal digestion by interfering with the proper production of digestive enzymes, but also produces abnormal breakdown products of ingested foodstuffs, such as abnormal amino-acids, abnormal in quality and quantity, besides aromatic amine compounds, which produce rise of blood pressure. In addition to the chronic infection in the intestinal tract, there can be no doubt that microbes from the foci of infection, both primary and intestinal, are passing

continually into the circulation. Quite commonly such microbes are excreted by the kidneys, and can be isolated from the urine.

There are thus continuously passing into the blood products from normal and abnormal digestion and micro-organisms. As a rule, much more protein than is necessary for nitrogenous equilibrium—that is, for the maintenance of the nitrogen element in the structure of the protoplasmic molecule—is ingested. The superfluous amino-acids to which it is reduced are deaminised, in the intestinal cells especially, and in the muscles, and the resulting ammonia is synthesised into urea in the liver.

In fatigue of the muscles and in abnormal metabolism produced by fever, where there is anoxæmia, acids are formed, such as sarcolactic acid, and these, getting into the circulation, interfere with the normal metabolism, since the intracellular and tissue ferments work best in an alkaline medium. Anything that leads to an increase of acids from the intestine, such as are produced from deaminised amino-acids will aggravate the condition, or any poison which interferes with oxidation in the tissues, such as alcohol. Again, the interference with the oxidation in the tissue cells may be due to microbial infection. I have had one patient in whom an acute exacerbation of gout was brought on by over-indulgence in tobacco.

In all these conditions an abnormal amount of uric acid appears to be formed—and it is not—as it is normally—changed into the harmless urea. In gout, it is deposited in the inflamed tissues in the form of salts.

To sum up, the original condition appears to me to be produced by microbes from a focus of infection, supplemented by secondary infections, and is maintained by vicious circles produced by the abnormal metabolic products formed—(*a*) in the intestine, (*b*) in the tissues.

Such is the hypothesis on which the treatment of these conditions is founded. If it is correct, then removal of the infecting focus and reduction of the action of the microbes in the intestine ought to produce cure if the tissues affected are not too damaged to allow of this result. And this I have found to be so. By the methods described below several cases of typical gout have been cured.

Prophylaxis.—As in the acute cases, the prophylaxis of chronic rheumatic joints, &c., consists in the clearing up of the sources of infection that produced them. The first twinge of rheumatic pain should be sufficient warning to the patient that he must have his microbial inhabitants investigated, his foci of infection cleared up, and his resistance increased to the microbial inhabitants of his intestine.

Treatment.—Consists in inoculation with a vaccine made from the microbes found in the focus of infection with a vaccine made from the patient's coliform bacilli and with a vaccine made from any microbe, other than *B. coli* or streptococci, which is being excreted from the patient's urine. If staphylococci are being excreted by the kidneys, streptococci isolated from the gums, and coliform bacilli from the intestine, the series of doses would be in the order mentioned :—

25	million	+	2½	million	+	2½	million.	
50	,,	+	5	,,	+	5	,,	
75	,,	+	7½	,,	+	5	,,	
100	,,	+	10	,,	+	10	,,	
150	,,	+	15	,,	+	15	,,	
200	,,	+	20	,,	+	20	,,	and so on up to
2,000	,,	+	200	,,	+	200	,,	or more

Not uncommonly other microbes than the streptococci are responsible for the primary infection. In several cases

I have found Gram-negative bacilli, not described, in the gums or being excreted in the urine. The initial dose of these is 2½ millions.

One such bacillus was motile, it did not liquefy gelatine. It produced acid, but no clot in milk. It produced acid and gas in glucose, saccharose and mannite. It produced acid very slowly, but no gas in lactose. Another, feebly motile, produced acid in saccharose and glucose. It did not ferment lactose or mannite in five days, and produced acid and clot in milk. Another, non-motile, produced acid in lactose, saccharose and glucose, but not in mannite, and acid and clot in milk. So that many different kinds of these Gram-bacilli appear to infect.

The correction of acidosis is particularly important in the treatment of these conditions, and the control of reactions by iodine therapy most helpful.

Muscles.—Muscular rheumatism or fibrositis is investigated and treated in exactly the same manner as that of the joints.

A NOTE ON FIBROIDS AND PREGNANCY.

By BETHEL SOLOMONS, M.D., F.R.C.P.I.;
Gynæcologist to Mercer's Hospital, Dublin.

[Read in the Section of Obstetrics, December 8, 1916.]

I do not wish to enter in this paper into a dissertation on fibroids complicating pregnancy, but merely to raise a few points which have struck me whilst operating during the course of the past few years.

In dealing with this subject the first question which arises is—do myomata cause sterility? In Kelly's series about 50 per cent. were sterile. If one is to judge by the number of cases of sterility which one meets, which are due to causes other than myomata, one is inclined to answer that while the presence of myomata causes sterility, the latter state is more frequently caused by other factors, such as tubal disease, &c. If one is consulted by a woman who wishes to become pregnant and in whom one diagnosticates fibroids it is extremely difficult to decide on a diagnosis. Subserous and interstitial myomata are often removed with the object of curing sterility but except in the cases where there is marked displacement, the results of these operations are usually negative. Where the case is more difficult, and the myoma is found growing into or from the uterine cavity the removal of such tumours leaves the patient in a better condition for the pregnant state. Great care must be taken in these cases to stitch the endometrium back into its place before sewing up

the muscular walls of the uterus; otherwise severe and sometimes fatal hæmorrhage may occur. I have attended patients in their confinements in whom at least one-third of the cavity has been taken away with the tumours and I await the onset of labour after these large myomectomies with utmost confidence.

The next question that arises in regard to possible pregnancy after myomectomy is the suture material. I use No. 4 iodine catgut. It has been suggested that a parallel might be advanced in the case of Cæsarean section where most gynæcologists use silk. There is no similarity in the two cases. In the former there is no involution and if the operation is properly performed the uterine wound should heal by first intention: in the latter case there is of course a great strain on the sutures. I have found that the best results in myomectomy are obtained by stitching the peritoneal surface of the wound by means of continuous button hole sutures.

When pregnancy occurs in a uterus which contains myomata—what is the result? It is a well-known fact that myomata during pregnancy hypertrophy. Abortion or premature labour may terminate the pregnancy. If the tumours are situated in the pelvis they may become incarcerated and gangrene may result or even the rectum may get compressed. Red degeneration is a serious complication. Some give no trouble whatever. Submucous fibroids may become twisted and later gangrenous, or else they may become separated and get delivered. Although some authors have reported the complete disappearance of tumours, most authorities deny such a phenomenon.

It is impossible to lay down hard and fast rules for the treatment of myomata in pregnancy. The rational treatment can be summed up in a few words. If the tumour

is likely to give trouble during parturition, extirpate it. If not, leave it alone. The case which I am about to report demonstrates most forcibly one of the difficult problems of obstetrics. The following is the history :—

Case.—Mrs. B. McG., aet. 40, married 7 months, was referred to me by Dr. Flood, of Bundoran, and was admitted to Mercer's Hospital on September 12, 1916. She was an anæmic-looking woman and had no previous medical history beyond the fact that she had had Alexander-Adams operation performed in America two years before. She had consulted her doctor because of very great pain in the left side. The menstruation had been of 28-day type, lasting seven days, and rather heavy. The last menstruation occurred on June 17, 1916. On examination the diagnosis of three months' pregnancy, complicated by myomata, was made. I advised the patient to have a laparotomy performed, and informed her that I would endeavour to remove the tumours and leave the pregnant uterus, but that the necessity for removing both uterus and tumours was probable. I made clear the fact that after opening the abdomen, I might find it necessary to close it, and let her chance going to full term. On opening the abdomen myomectomy was found to be out of the question, while her chance of carrying the baby to full term was impossible; therefore the uterus and a cystic right ovary were removed, whilst the left ovary, which contained small cysts, was resected. The convalescence was uneventful, and the patient left the hospital in a fortnight. The specimen consisted of a three months' pregnant uterus containing myomata, some of which invaded the cavity. There were several with pedicles, but the majority were incorporated in the uterine wall.

In a case such as this there was no other course open, as myomectomy was impossible. If nothing had been done, what would have happened? (1) She might have

aborted. (2) She might have gone to full term, when the necessity for performing Cæsarean hysterectomy would have arisen. (3) Any of the other complications already mentioned might have arisen. Her chance of going to full term was very remote. Apart from other considerations, the left-sided pain had become nearly intolerable, and the examination of the specimen showed the impossibility of such a proceeding. The treatment by *x*-rays would not have had any effect in a case of this kind, but the rays might have brought on an abortion. If myomectomy is performed during pregnancy, the consensus of opinion is that although the operation is often desirable, the chance of miscarriage is very great.

In labour at full term myoma may act as a grave complication. When situated in the cervix, it may definitely obstruct; when in the body, it may cause inertia and *post-partum* hæmorrhage. If the myoma is cervical and pedunculated it may be removed, or it may be pushed past the presenting part. If sessile, the danger of performing myomectomy is marked, because of hæmorrhage, and Cæsarean hysterectomy is the operation of choice. If the myoma is corporeal, it should be left alone unless there is dystocia, when it should be removed if pedunculated, and Cæsarean hysterectomy should be performed if sessile.

During the puerperium the myoma may slough, or red degeneration may occur. In the presence of any degeneration hysterectomy should be performed.

To sum up the conclusions from this short note :—

1. When myoma causes sterility it is usually submucous in variety; but myoma is one of the least common causes of sterility.

2. When performing myomectomy with pregnancy in view, if the edges of the wound are approximated carefully

with No. 4 iodine catgut, the confinement may be approached with confidence.

3. After myomata are removed during pregnancy, miscarriage is a common outcome. When multiple fibroids complicate pregnancy hysterectomy is usually necessary.

Sir William Smyly said that it was generally believed that myomata caused sterility, and though that belief was probably correct, yet, so far as he knew, there were no statistics to prove it. The disease usually occurred at a time when women were less fruitful than earlier in life, and, therefore, it would not be sufficient to show that women with myomata were below the average in fertility. As regards operating during pregnancy, it should, and generally could, be postponed if possible until after delivery. But during the past year he had been obliged, on account of excessive pain, to enucleate a tumour as large as a billiard ball at the sixth month, and the patient subsequently went to full term, and was confined in the Rotunda Hospital.

Dr. Alfred Smith said one of the many interesting points raised in Dr. Solomons' paper was whether fibroids caused sterility. Fibroids undoubtedly have some influence in causing sterility, but not so much as is generally supposed. Operations during pregnancy had no terrors for him if the proper time was selected—*i.e.*, the time of the month corresponding to the mid-menstrual period, as the uterus is then less irritable. He recalled a case of a sixth months' pregnancy where he had to perform a hysterectomy owing to distress from the pressure of a large fibro-myoma.

Dr. Spencer Sheill considered that great care should be taken in explaining to women the results which followed hysterectomy.

The President said that obviously myomectomy was the proper operation when fibroids were found complicating pregnancy and causing symptoms necessitating interference, or where they were so situated as to render the probability

of delivery at term doubtful. But when the fibroids were multiple and intramural, there was always the risk that the evacuation would leave the uterus in so mutiliated a condition as to render its retention unjustifiable, and then hysterectomy was the only alternative. The point must be left to the judgment of the operator at the time of operation.

Dr. Solomons agreed that it was most important that patients, whether in a general hospital or in a private home, should be told most carefully the after-effects of operations which involved the removal of important organs. In the case mentioned in his paper the woman and her husband understood all the possibilities, and were most anxious to leave the treatment to him (Dr. Solomons). He did not think that multiple myoma contraindicated myomectomy. Sometimes the removal of one myoma was more difficult than of many. He thought that operations during pregnancy were undesirable unless when necessitated by exigency.

A FACTOR OF USE IN ESTIMATING CRANIAL HEIGHT.

By J. R. D. HOLTBY, M.B., B.S.;
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[Read in the Section of Anatomy and Physiology, December 15, 1916.]

Some time ago I received some old Irish skulls for examination and report. As the greater portion of the base was missing in each case it became a question as to how the altitudinal index was to be estimated. For this purpose the basio-bregmatic height is required, and only the opisthionic portion of the foramen magnum remained. It, therefore, occurred to me to inquire if there was, as one would expect, any constant relationship between the basio-bregmatic and opisthio-bregmatic heights. This communication deals with the results obtained.

Thirty-five skulls were examined. These were collected from various sources, so as to give as representative a view as possible. The series contained specimens of modern and ancient Irish, Australians, Hindoos, Siamese and Peruvians. Three young skulls were included in the survey. The method adopted was to estimate the cephalic index, so as to ascertain the type, then to measure the actual basio- and opisthio-bregmatic heights. The proportion between these two latter figures was estimated to two decimal places and the mean taken of these proportions. In this way was obtained a mean factor, which,

when multiplied by the opisthio-bregmatic height, should yield the basio-bregmatic measurement.

This mean factor was 0.9028, or for practical purposes 0.903. Roughly, therefore, the basio-bregmatic measurement is 0.9 of the opisthio-bregmatic.

This factor was now used, and the calculated basio-bregmatic height compared with the actual measurement.

For example :—

Opisthio-bregmatic height, 150 m.m.
Calculated basio-bregmatic, 135.54 m.m.
Measured basio-bregmatic 135. m.m.

In 15 cases out of 35 the factor 0.903 gave an absolutely accurate result. In 13 other examples the error was only 2 m.m.

In the remaining seven cases the error would have been 4 m.m. in five examples, and 7-8 m.m. in three cases. The basio-bregmatic height is said to be in any one race the least variable cranial measurement, though it frequently varies up to 10 m.m. The greatest error, if it had been calculated by this factor, would be 8 m.m. (well within the normal range of variation), and this would only occur in about 6 per cent. of cases.

One would imagine that the relationship between basio- and opisthio-bregmatic measurements might vary with two things—

1. Inclination of the foramen magnum. In Europeans this is inclined downwards and forwards; in Australians, downwards and backwards from the horizontal.

2. Position of the bregma, which will vary in long and broad skulls, according to the direction in which the chief growth took place.

Inclination of the foramen magnum makes a very small difference: the greatest error obtained by use of this factor in the case of Australian specimens being frequently reached, or even exceeded, by European examples. The error in the former cases was only 2 m.m.

Influence of Shape of the Skull.—The smallest number of errors was obtained in dolichocephalic or long-headed specimens. In mesaticephalic examples the error was sometimes 2 m.m., either too much or too little.

Reasonably accurate estimations may be made by use of this factor in dolicho-, mesati-, and brachy-cephalic skulls; though extreme errors, such as 6-8 m.m., are found in each group. I am not satisfied that the shape of the skull has any definite and invariable influence on the factor.

Conclusions—1. By use of the factor here given (0.90) we can readily estimate the basio-bregmatic height of a skull if the opisthio-bregmatic measurement be given. The method is thus valuable in the examination of old or rare specimens, which are so often in a damaged condition when received.

2. The error in the result obtained by this method is usually very small—only 2-3 m.m., and in no case did it exceed 8 m.m.

3. The factor can be used for any skull, and even holds true in the case of specimens from children.

Appended are some measurements showing—

(*a*) The cephalic index.
(*b*) The true factor in each particular case necessary to give the measured basio-bregmatic height.
(*c*) The measured basio-bregmatic height,

(*d*) The basio-bregmatic height as calculated by the use of the factor 0.90.

(*e*) The source of the skull, so far as was known.

a Cephalic Index	*b* True Factor	*c* Measured BB height	*d* Calculated BB height	*e* Nature and Source
88	0.94	141 mm.	135.5 mm.	Siam
69	0.92	124 mm.	121 mm.	Australian
75.8	0.90	128 mm.	128.7 mm.	Old Irish
76.4	0.90	128 mm.	128 mm.	Old skull from Howth
80	0.90	128 mm.	128 mm.	Modern Irish
70	0.90	135 mm.	134.1 mm.	Australian
77	0.93	138 mm.	132.7 mm.	Hindu
—	0.90	118 mm.	117 mm.	Young modern Irish
—	0.90	111 mm.	112 mm.	Young Peruvian

The chief value of the altitudinal index lies in its comparison with the breadth or cephalic index, the figures usually being much the same in each, so that a dolichocephalic skull (below 75) would usually show an altitudinal index of below 75. The maximum error obtained by calculating the altitudinal index by this method would be 5 (five); a deviation from the correct figure which would not be likely to cause any erroneous conclusions to be formed.

A THEORY OF THE BODY TEMPERATURE.

By J. M. O'CONNOR, M.D.;
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[Read before the Section of Anatomy and Physiology, December 15, 1916.]

The body temperature—a matter of weighty practical moment—has as a branch of physiology been singularly free from speculation. Its remarkable constancy was noticed early; it is even stated that it was used by Fahrenheit as one of the first fixed points on his thermometer, to be soon discarded in favour of the temperature of boiling water. It has been long ago shown that the boiling point of water is in no sense an absolute, but the body temperature still remains as something intrinsically determined for the healthy organism. Fever, it is true, shows us every day that it is capable of alteration; but by a natural prejudice the pathological is seldom regarded as other than a disturber of order, and the increase of temperature in fever as something added on to the normal. As a result we have, in spite of the admirable pioneer work of Liebermeister, no attempt—so far as I know—at a theory of the body temperature beyond the view that it is an evolutionary remnant.

During experiments on the question of the regulation of temperature I have met facts which have induced me to consider whether all maintained body temperatures classed as normal or abnormal by applied physiology cannot be explained on the same grounds. The very fact

that speculation on the subject has been so rare has driven these theorisings perhaps too far beyond the bounds of ascertained fact. It has, however, been pointed out to me that, correct or not, they have the merit of endeavouring to remove the body temperature from the axiomatic; and that, as the experimental proof requires much time and meets many difficulties, a sketch of a possible solution is justifiable.

That the temperature of an animal is at all above the average of the moist external air is a natural result of the conversion of chemical energy into motion. Some classes of animal life have, in addition, the power of keeping their deep temperature fixed in spite of variations in the outside conditions. It is easy to find advantages in this, but such teleological matters do not concern us; the point we have to deal with is why this temperature is a particular one.

It is well known that the body temperature is maintained by two mechanisms : on the one side by increasing the production of heat; on the other by availing of mechanisms to facilitate its dissipation. The first, called by Rubner [1] the chemical heat regulation, is easily investigated. If we expose a fasting animal to various air temperatures we find that the heat production, fairly measured by the oxygen consumption, falls gradually with rising air temperature until it reaches a minimum at about 25° C. Below this minimum—called the basal metabolism—it cannot be brought. Further increase in the air temperature produces—speaking generally—no alteration in the metabolism until the mechanism of cooling reaches the limit of its efficiency. When we turn to this other, or cooling mechanism—" the physical heat regulation "—we are not so satisfactorily placed. So far as I

know, the only attempt which has been made to express the *mechanism* of heat loss in figures has been Rubner's determination of the water loss by evaporation. This in the dog is at its minimum at about 15° C., but alters rapidly only during the period of air temperature in which the basal metabolism is stationary. The results, however, are for this purpose clearly unsatisfactory. A large portion of the loss comes from the lungs, and is, consequently, mainly at the mercy of the respiratory needs, and of that which is lost from the skin a considerable portion is due to a passive drying, occurring in the dead as in the living, influenced largely by the temperature of the skin itself,[2] and in no sense to be considered as representing a biological activity. It is fairly certain, however, that there is a point of outside temperature at which the animal is at its minimum heat production, and at which there is no active effort made to accelerate heat loss at which, in short, the passive heat production of the animal is in balance with its passive heat loss.

Let us consider to what this minimum heat production is due.[3] It is well known that this basal metabolism is in closer relation to the area of the different animals than to their mass, and a tendency has consequently arisen to divide it into two parts : the heat production by the necessary mechanisms of resting life—the heart and respiration, to mention the obvious—and a second heat production *per se* necessary to maintain the body temperature at a height fixed by causes not to be examined. Of recent years, however, the view has been brought to notice (Dreyer) that the basal exchange in different animals varies in proportion to other things—such as the cross-section of the trachea and of the aorta, with which a much more legitimate causal relationship can be traced. It may be

pointed out incidentally that the conception of metabolism as a decomposition of the actually living, and by implication withdrawn from the law of mass action, is gradually fading, and the possibility of expressing physiological combustion processes in terms of the concentration of the material actually decomposed is becoming clearer. Modern tendency, then, moves towards regarding the heat production of the body as a sum of similar processes resulting from the functional activity of the different organs. The intensity of such chemical processes is influenced by the temperature at which they occur—generally, the higher the temperature the more active they are—and this influence is easily noticeable in cold-blooded animals—in the frog, for example, where the graph of heat production for various temperatures is approximately a straight line from about 22° C. on. That the heat production of warm-blooded animals is similarly influenced can be shown by heating them above their normal temperature, when we find their heat production rising proportionately. An effort to investigate their basal metabolism at lower temperatures is complicated, if not frustrated, by "chemical regulation." In animals which are anæsthetised the regulation point is lowered, and no matter what it be, heating the animal to higher temperatures causes an increase in metabolism, approximated in a graph, as it is in the cold-blooded animal, by a straight line.[4] We are consequently justified in claiming that the heat production of an animal which is not regulating is a function of its temperature. If we know the heat production (a) at a temperature T, and the gradient (b) of the graph of heat production and temperature, then the heat production at a temperature T_2 will be—$a + b\ (T_2 - T_1)$. Suppose T_2 to be the temperature for which the animal regulates. Now we know that

there is a temperature of the outside air with which this heat production is in passive equilibrium. We must then turn to the heat loss of this outside temperature t_0. Heat is, as a rule, lost by conduction and radiation and through evaporation. Naturally, however, the body temperature can be maintained under conditions which admit of heat loss by conduction to external air alone, and we may limit our considerations to this simple case, ignoring the small quantity of heat lost by warming inspired air. The quantity of heat lost will then be proportional to the difference between the temperature of the body and outside air $(T_2 - t_0)$, and to the surface of the body s and the conductivity (assumed uniform) of the skin and its covering appendages, and will be inversely proportional to the depth d to which we must go to meet this uniform body temperature—*i.e.*, equal to—

$$\frac{sc}{d}\,(T_2 - t_0)$$

In addition, however, to conductivity the escape of heat is assisted in the deeper layers of the skin by what we might call the convection element supplied by the capillary circulation in the skin. This will again probably be some function of T_2, and be also influenced by t, but we can express it for the moment in a given animal, a constant K. If then the animal is in equilibrium with its outside temperature,

$$a + b\,(T_2 - T_1) = \frac{sc}{d}(T_2 - t_0) + K$$

Now we know that the body regulates so that its subcutaneous temperature is the same as the body temperature for which it is regulating,[4] until, in other words, the temperature-perceiving mechanism in the skin is at T_2. As this mechanism is fixed in its position, d in the above

equation is fixed, and the only variables in a given case would be T_2 and t_0. If one of these be fixed, the other is also determined. We have no reason to believe that T_2, the internal temperature of the animal, is the factor that is fixed by causes outside the equation. We can, on the other hand, conceive several ways in which t_0, the outside temperature factor in the equilibrium equation, could be fixed. For example, it could be true the t_0 was the temperature, or a close relation to it, of the sea at the time when the warm-blooded animal first evolved from cold-blooded ancestors. This theory does not appear to me the most probable, but I mention it as it is the one which in the absence of relevant facts requires no new assumption. Further discussion on this point is unprofitable, as is also a consideration of the influence of the specific dynamic effect of foods. If then we consider t_0 fixed, it is clear that so long as the basal metabolism, the influence of temperature on it, and the circulation of blood through the skin remained unaltered, the body temperature to which the animal regulated would be constant. If, however, the basal metabolism changed or the normal circulation through the skin altered without proportionate mutual influence, the temperature of the animal would alter also. On these lines the phenomenon of hyperpyrexia and the influence of narcotics on the body temperature would probably fit without obscurity into the scheme.

REFERENCES.

[1] Rutner's work can be found summarised in his book Gesetze des Energieverbrauchs bei der Ernährung. Leipzig. 1902.

[2] O'Connor. Journal of Physiology. Vol. 49. P. 113. 1915.

[3] See Krogh's Monograph on Respiratory Exchange. 1916.

[4] O'Connor. Proceedings of Royal Society. B. Vol. 89. P. 201. 1916.

NEPHRITIS.

By WILLIAM BOXWELL, M.D. Univ. Dubl.; F.R.C.P.I.;

Physician to the Meath Hospital and County Dublin Infirmary.

[Read in the Section of Pathology, January 12, 1917.]

It is not proposed in this paper to touch on any forms of nephritis of known bacterial origin. These conditions are understood. Bacterial affections of the kidney behave very much as similar affections elsewhere. The pathological findings are pretty constant, and are what one would expect to find, and the effects of bacterial invasion are largely local.

With ordinary non-bacterial nephritis it is quite otherwise. In most cases the ætiology is wrapped in mystery. Cases of acute nephritis following definite chills, or poisoning by drugs like cautharides, are comparatively rare. In most cases the disease comes on for no definite reason in people previously in perfect health, and no man can foretell with any certainty what the result may be. Once in a way it follows an extensive burn, though I do not remember having seen such a case. Of the infectious fevers, scarlatina and diphtheria are most prone to act as predisposing causes. I have seen it complicating ichthyosis.

In the early acute stages the condition of the urine and the *post-mortem* findings correspond pretty well.

The urine is reduced in quantity, highly concentrated, throwing down a heavy precipitate of urates, uric acid,

and sometimes calcium oxalate. It is frequently mixed with blood, gives a moderate amount of albumen, and shows tube-casts which are hyaline or granular, and in which are embedded red blood cells, leucocytes, epithelial cells—whose nuclei still stain, but rarely fat globules. If such fatty casts are present they are few and far between. *Pus cells are not uncommon.* The kidney itself is red or pale, rather enlarged and smooth, and the capsule peels readily. On section the cut surface is often flecked with hæmorrhagic spots, and tends to bulge, due to swelling from congestion and contained exudate, and the practised eye can sometimes detect when the kidney is quite fresh —the "ground-glass" opacity of cloudy swelling.

Microscopically we see all the cardinal characteristics of an acute inflammation. Vascular change; the exudate, lying coagulated within the Bowman's capsule, with the cellular elements, congregated in the capillary loops and round about the smaller blood vessels; the tissue degeneration in the swollen and desquamating epithelial and endothelial cells, with here and there commencing fatty degeneration, and sooner or later cell proliferation. Such is an average finding when one is lucky enough to obtain the kidneys for examination where death has occurred in the early stages of the disease.

Fortunately, many of these cases get well, especially those whose onset can be traced to a definite cause. I think the prognosis is distinctly better in an acute case, brought on, for instance, by leaving a fly-blister in contact with a broken surface than in another worse similar case coming on insidiously.

If such a case fails to recover it passes into the second variety—subacute nephritis, more commonly known as chronic parenchymatous. Before death occurs, which

usually does within a year or two, either from exhaustion, dropsy, intercurrent infections, or in the non-dropsical cases from uræmia—this stage of the disease is recognised from the pathological changes in the urine. It is moderate in amount, not very far removed either way from the normal, very pale, of low specific gravity, turbid from the presence of fatty tube-casts, and wholesale desquamation of the disintegrated renal epithelium, and holds albumen in enormous quantity.

The kidney one expects to find, according to the accepted teaching, is a large pale or mottled one, with an undulating surface, on which the stellate veins sometimes stand out distinctly.

The cut surface should be pale, grey, yellow, or putty-coloured, with a wide cortex, and usually dull red pyramids. The translucence of the normal kidney is gone; the capsule peels abnormally easily, being partly separated by exudation; the vessels are not obviously thickened or protruding; an occasional small cyst may be found deep in the substance of the kidney.

Microscopically, the hyperæmia is present, but one has to look for it. Parenchymatous and fatty degeneration are profound and widespread. The epithelial structure of the tubes is a wreck. Many of the tubes have no epithelium left at all, and the basement membrane—slightly thickened and œdematous—forms in places an empty net-work, lodging here a mass of disintegrated epithelial cells, there a tube-cast.

Of connective tissue proliferation there is just the amount one might expect from any inflammatory process prolonged over some considerable time. There may be some little thickening of Bowman's capsule, some cell infiltration or proliferation of the cells of the glomeruli, but

there should be none of the massive cell proliferation, arterial thickening and general fibrosis characteristic of the true chronic interstitial type. Such, I think, is a description of the "large white kidney"—near enough, at any rate, to be recognisable. And such kidneys do occur; so much is usually allowed.

What is not recognised, perhaps, so generally is the extraordinary variability of the pathological findings in cases which have been clinically identical.

One cannot say with any certainty what a kidney is until one cuts it. On the table are kidneys, with their corresponding microscopic sections, which are not brought here because they are particularly good examples of any special variety. They happen to be kidneys whose clinical history I knew. I watched the progress of these cases in hospital, and made sections myself of the kidneys after death.

In the first group, clinically these cases were typical cases of subacute nephritis, with the classical signs and symptoms. They had all pale kidneys, but two of them are amyloid; one shows well-marked, though comparatively early, interstitial nephritis; and the fourth, which was from a case of typical acute to subacute nephritis of four months' duration, in a young man of 18, with scanty blood-stained urine and mixed casts, many of them blood casts, proves on section to be a hardened old chronic interstitial nephritis, with advanced fibrotic and hyaline changes.

One does sometimes find in cases of "subacute nephritis" of unusually long duration a "small white kidney," which shows the profound parenchymatous changes of the larger variety, together with the fibrosis inseparable from all inflammatory conditions lasting a long time.

It is generally believed that subacute nephritis may be a subacute condition from the very commencement, without any antecedent acute stage.

The third type is the kidney of "chronic interstitial nephritis." The kidney is known by a large variety of names—the "granular contracted," the "small red," the "morocco leather," the "cirrhotic," the "gouty," and the "arterio-sclerotic" kidney.

These kidneys may be of almost any size up to normal and almost any colour—from a deep plum colour to a dirty yellow or grey.

But whatever colour or size may be present they all have certain fixed characteristics.

They have a rough surface, they have an adherent capsule, a cortex sometimes reduced to vanishing point, and sclerotic arteries, which stick upon the cut surface like crow-quills.

Microscopically, they are all fibrous. Recent inflammation is marked by dense small round-celled infiltration. The evidence of old disease is hyaline or fibrous changes, mainly in the glomeruli, Bowman's capsules, and the blood-vessels. The condition of the epithelium is quite variable. In some cases it appears to be atrophied rather than grossly degenerate: that is, the cells are cubical or flat, but the nuclei stain brightly, and are regular and distinct.

In other cases the epithelium is obviously in a state of advanced fatty degeneration. The tubules may be packed with hyaline casts. What one would like to know is the antecedents of these weather-beaten looking organs. Many of them certainly have had a "past."

But that they are all examples of the clinical disease known as "Chronic Bright's Disease" I do not believe.

How many of them are the wrecks of an early encounter with one or other of the causes of acute tubular or glomerulo-tubal nephritis? How many owe their cirrhosis to bacterial infections, latent, and quite unsuspected by their owners?

One of the most typical examples I ever saw of this kidney was removed by a surgeon because he did not know what else to do with it, and the patient was none the worse. He had been led to expect a large pyonephrotic kidney on the evidence of an *x*-ray and a history of a mild pyuria of unknown duration. The patient, a young woman about 24 or 25 years, complained of pain in the loins, coming on pretty suddenly, and accompanied by vomiting. I found constantly a little pus in the urine, but no tubercle bacilli. I did not get the opportunity of looking for "B. coli."

Later circumstances, over which neither she nor I had control, brought her under the care of a surgeon, who decided to explore the right kidney, and finding, to his disgust, instead of a large suppurating organ, a very small granular contracted one, he, to my amazement, took it out. There was no reason to suppose that the other kidney was not in the same condition. No attempt to find out anything about it had been made, and I expected to hear of the patient's early demise from uræmia. But she is alive and well, and working.

This patient had had scarlatina rather later than usual —when she was 14 or 15—and at the time her doctor was anxious about her kidneys, examining the urine frequently. But whether she ever had nephritis actually she did not know. Possibly this kidney has been the subject of that obscure affection unknown to clinicians, but described in pathological works as "acute interstitial nephritis," and

found chiefly in people suffering from scarlatina and diphtheria.

Is there any reason why a fibrosis once set going should not continue in the kidney, just as it continues to advance in mitral stenosis long after all the acute process has subsided?

Now, as regards the relation between the clinical disease, chronic interstitial nephritis and acute or subacute tubular nephritis, I do not believe there is any. That is, I do not think that the stages are acute, subacute, and chronic interstitial. I think this last is an entirely separate entity as a disease. *One* of its characteristics is a cirrhotic kidney; but all cirrhotic kidneys do not connote chronic Bright's disease.

This condition hardly ever occurs before 40. It is frequently found active in people who give no history of antecedent kidney trouble of any sort.

It is found equally in the idler and the worker; in the toper and the teetotaller. I do not know that alcohol has any more to say to its causation than it has to the causation of cirrhosis of the liver; and the same may be said of syphilis. I know a man who has been an intermittent drunkard since he was 18, and had syphilis when he was 25. He still has arteries like a child, and he is over 45; and he can walk a mountain, ride to hounds, and shoot driven grouse with any man, and he lives like a lord.

Pathologically, this condition is characterised by high arterial tension, sclerosed vessels, and generally an enlarged heart. I have already mentioned the chief points about the kidney in the advanced stages. The cirrhosis begins, as it does in the liver, with rather patchy small-celled infiltration. It is the primary response of the connective tissue to a feebly toxic irritant. It can be seen coming on

before there is any noticeable change in the epithelium, and the epithelium often remains clear and brightly staining long after fibrosis is well established in the immediate neighbourhood. It is a pathologically chronic condition from the outset; and, as always, time is simply an accidental attribute. As pathological terms, acute and chronic have reference to tissue changes of a definite type, and inflammation may be acute, and remain acute for months; whereas, the chronic change may begin as a chronic stage from the very outset, and its "hall mark" is connective tissue proliferation. Parenchymatous changes may be quite secondary to it, or be there by the sheer accident of an intercurrent toxæmia of a different order.

The urine commonly found is what one might expect—abundant, pale, generally quite clear, without visible deposit, although hyaline casts may sometimes be collected in fair quantities by the centrifuge, of low specific gravity, and with albumen in relatively minimal amount.

Dropsy is conspicuous by its absence until the heart fails, and uræmia is always in the offing.

I believe the whole disease is generalised toxæmia, acting mainly on the vascular and renal systems, and producing there its most obvious pathological changes; and I do not think that the extent of the renal change has any more definite relation to the severity of the disease than the depth of an ulcer has to the severity of an attack of typhoid fever.

The foregoing observations simply represent my own attitude towards these puzzling diseases—an attitude having its origin partly in past teaching and partly in clinical experience, followed up by *post-mortem* examinations—naked eye and microscopic, whenever possible. No

attempt has been made to collate recent views upon the subject from the newer text-books, nor is it claimed that there is anything whatever original in anything I have stated.

Dr. W. M. Crofton gave an account of some cases with subacute and chronic nephritis, in which he had examined the bacteriology of the urine, and most of which cases he had treated with vaccines or microbes isolated from the urine. Most of the cases were treated in Malta during his service there, and when the patients were convalescent they were sent home, so that neither the case-sheets nor the after-history of the patients are available.

Case I.—Lance-Corporal C. Had been in hospital some time, invalided from Salonika with nephritis. He suffered from headache, œdema of the legs, &c. He had a marked amount of albumen and casts and bacilli in his urine. A vaccine was made, and he received a series of doses. When he left the hospital the albumen was reduced to a trace.

Case II.—Private L. Had a very similar story. He had albumen and casts in his urine. He was treated by vaccines. Had only a trace of albumen on leaving hospital.

Case III.—Private D. Had been in hospital from Salonika with subacute nephritis. A Gram-negative non-motile bacillus was isolated from his urine. The albumen was markedly reduced after the first injection.

Case IV.—Private C. Was invalided from Salonika with nephritis. He had the pale, puffy face of a nephritic, and had a considerable amount of œdema. Staphylococcus aureus was isolated. He was very markedly improved. Little œdema in his legs and a marked reduction of albumen.

Case V.—Private B. Also had large numbers of staphylococcus in his urine. The amount of albumen in the urine

was certainly very much reduced. He went home very markedly improved.

Case VI.—Private W. Subacute nephritis, with a slight temperature. Albumen, tube-casts, and some pus cells, and bacilli in his urine. Vaccines were made. The urine became completely free of albumen, casts, and microbes.

The Malta Cases.—All had been under ordinary treatment for a considerable time in hospital before vaccine treatment was applied, so Dr. Crofton considered that the vaccines must have had something to do with their improvement. The only conclusions he wished to draw from these few cases is that this is a hopeful line of investigation and treatment of Bright's disease.

Dr. Drury said it was much to be regretted that there had been so little real advance in our knowledge of this most serious disease since the days of Bright; and he considered that this was largely due to the separation of the clinician and the pathologist, which was growing greater every day. This was necessary, but unfortunate; for in such a large subject, unless investigation was confined by certain men to one single line, whether it be to elucidate the cause or explain certain symptoms, no advance could be expected. At present if the clinician sent the organs of two cases, quite dissimilar as to their symptoms, all the information he got back was "chronic nephritis" (which he already knew) and information as to the amount of cellular or fibrous elements present (which did not help him).

He thought, though, that too much was expected of the pathologist and bacteriologist. Probably more information would be got through the medium of bio-chemistry, in which branch little had been done yet.

This, too, might yield us aids to prognosis, which we now sadly lacked. There was only one aid which he knew of that could be counted on—that was albuminuric retinitis, which in his experience meant that the case was very near its termination. Another symptom had been referred to as having

much the same prognostic value—namely, dyspnœa, with which he disagreed; and he briefly outlined a case in which it was severe and the sole cause of the patient seeking medical advice. It disappeared, or it was kept in abeyance for over two years, and then, on reappearing, the patient was persuaded that it was cardiac, and was so treated till it became so bad that he sought other advice. Another symptom—headache—was surely more likely to be explained by the bio-chemist than by the pathologist. He had his own explanation, but it was pure theory, yet it was all there was to act on in treatment. It was, he believed, a symptom of grave import, not as to duration, but of the probable sudden termination of the case.

Trench Nephritis.

Dr. T. Gillman Moorhead gave a short description of this affection, with several references to some cases that had come under his notice in a military hospital in Egypt, and also in his wards at home.

He pointed out that the usual early symptoms consist of transient œdema, dyspnœa, and albuminuria. In some cases also there was distinct cardiac dilatation with cyanosis. Usually, at the onset, there is a febrile period, lasting about three or four days. The ætiology of the condition has been much discussed, but the only thing that seems at all settled is that the disease must be microbial, as the onset and symptoms are exactly similar to those met with in known acute infections. The prognosis in most cases is good, several patients having completely recovered in three or four weeks from the commencement of their illness; in others, the albuminuria persists after all other signs have gone, and in his own experience several cases of persistent hæmaturia have also been encountered. As regards treatment, he had found the diets of Langdon Brown particularly suitable; complete rest at first, modified rest later on, and general tonics were the only other things required.

Dr. A. R. Parsons detailed the findings in several cases of nephritis which had come under his notice. He mentioned

some of the difficulties which arise in connection with the examination of persons for life insurance, particularly those persons who, though apparently healthy, nevertheless exhibited albumen in the urine. He considered that catheter specimens in the case of female patients should always be examined, but in some cases circumstances did not allow of such specimens to be taken.

Dr. F. Kennedy Cahill stated that he had found that the amount of albumen excreted by patients suffering from so-called physiological albuminuria was very variable.

Dr. Rowlette hoped for results from bacteriological research into the origin of nephritis. Acute nephritis presented so clear resemblances to known infective diseases that one could hardly believe that its cause was not micro-organismal. With regard to some forms of chronic nephritis the case might be different. In bacteriological study of the urine he thought it necessary to obtain the specimen by a ureteral catheter before deciding that the organisms which might be found came from the kidney.

Dr. Seton Pringle emphasised the importance of strict asepsis in the collection of urine samples for bacteriological examination.

The President said that he quite agreed with Dr. Drury in regretting the divorce of the clinical from the pathological study of such difficult conditions as nephritis. But the more deeply the analysis of the cases was pushed, and the greater the demand on time and technical resources, the less likely it was that the two aspects could be dealt with by the same individual.

So far as he could see, the best way to bring about the desired union of clinical with pathological experience would be to appoint whole-time professors of clinical medicine, debarred from private practice, and attached to hospitals provided with every requisite for pathological research. Such appointments had been made in the United States, and he hoped we would do likewise.

He inquired of Dr. Crofton the grounds on the strength of which he considered the organisms he had isolated from the

urine of his cases of nephritis were really infecting. Was there any serological or microscopic evidence? He thought that before one could fairly attribute the good results to the effects of vaccine therapy it would be necessary to compare the results with those in similar cases not so treated. Rest in bed and good food would, he thought, cause most such cases to clear up.

He agreed that in addition to bacteriological methods those of bio-chemistry and bio-physics should be more extensively employed in the elucidation of the problems raised by nephritis. An attempt should be made to ascertain systematically how in a given case the kidney discharged its three functions—filtration, secretion, and re-absorption. To do this properly the content of the patient's diet in water, salts, extractives, carbohydrates, and proteins would have to be exactly ascertained. He referred to the study of the tube-casts found in the urine of nephritis, and considered that the presence of blood-casts and epithelial casts indicated a severer process than the hyaline and granular varieties. In cases of trench nephritis which he had examined at the Castle Red Cross Hospital, he found chiefly the two former varieties together with many free red cells and free hæmoglobin in brownish granular form, and also rather more leucocytes than usual.

ABDOMINAL GUNSHOT INJURIES ADMITTED TO THE ROYAL CITY OF DUBLIN HOSPITAL DURING THE REBELLION.

By R. ATKINSON STONEY, F.R.C.S.I.;
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[Read before the Section of Surgery, January 19, 1917.]

When asked by the Secretary if I had any subject on which I could read a short paper this evening, I thought that it might be of interest to the members of the profession here to-night if I were to give a brief account of the cases of abdominal gunshot wounds that were treated at the Royal City of Dublin Hospital during the rebellion last Easter. The number of cases is so small, and the conditions found so varied that I am afraid that no very definite conclusions can be drawn from them, except that, perhaps, the first case emphasises the fact that no case should be looked upon as absolutely hopeless. From the experience gained in the present European war our ideas have undergone a complete change from those that were formed after the Boer War. The general experience then was that the best chance of recovery after abdominal gunshot wounds was treatment by rest, starvation and morphia, whereas operation was almost invariably fatal, and seemed to remove any chance of recovery that the patient might have had. As I say the experience of the present war is the exact opposite of this, the best chance afforded of the patient's recovery is immediate operation. This apparent contradiction of observed facts is probably due to several

causes. In the South African War most of the wounds were caused by rifle fire at long ranges, transport was difficult and delayed, operations could only be performed in adverse circumstances and often unsuitable surroundings. The soldiers in many instances had not had food for a considerable time before being wounded, and often the stomach and intestines must have been more or less empty. In the present war all these circumstances are changed, most of the wounds are produced by shell fragments or rifle bullets fired at close range, transport arrangements are much better, in many cases the wounded are brought to the casualty clearing stations within two hours of being hit ; and within a few miles of the firing line there are hospitals and surgeons fully equipped and capable of undertaking abdominal operations under the most favourable conditions. Finally, as the fighting has become more or less stationary, commissariat arrangements are almost perfect and the soldier well fed, so that the alimentary canal is not likely to be empty. In some places the French have built special dug-outs close to or in the firing trenches where abdominal operations may be undertaken, and so the patient operated on without delay. Here in Dublin during the rebellion the conditions were practically ideal for the treatment of the wounded, as in most cases they could be brought to a thoroughly equipped hospital and operated on by competent surgeons, as a rule, within an hour or so of being wounded ; and certainly in the case of general gunshot wounds the results were extremely satisfactory, and very little suppuration of a serious character was seen, in spite of the fact that many of the injuries were caused by ricochet or deformed bullets, fired at close range.

Curious to say, all the cases of abdominal wounds at the

Royal City of Dublin Hospital occurred among civilians. The following is a brief description of them :—

Here I wish to express my indebtedness to my colleague, Mr. Johnston, who has kindly allowed me to include the cases under his charge.

W. R. was admitted to hospital on Tuesday, April 25th, 1916, about 10.30 a.m. He had been shot about half an hour previously close to the railway bridge on South Lott's Road, and was brought to the hospital by the Pembroke Ambulance. There was a medium-sized wound of entry directly in the middle line at the level of the second lumbar spine, and a large ragged wound of exit, big enough to admit two fingers just below the right costal margin in the nipple line. There was also a large superficial lacerated wound of the right forearm, immediately below the bend of the elbow, evidently caused by the same bullet. The patient was brought from the ambulance into the special dispensary, which had been converted for the time being into a receiving and dressing room. As soon as his wounds were superficially examined, he was given a hypodermic of morphia, and immediately brought up to the operating theatre, and anæsthetised with ether. While the anæsthetic was being administered the anterior abdominal wound was washed with Eusol, and then the surrounding skin was painted with iodine. The man's condition was bad, the pulse was scarcely perceptible, and respirations were rapid and shallow; he was very pale, and the right side of the abdomen was dull on percussion. There was continuous oozing of blood from the abdominal wound, and from the condition of the man's clothing the hæmorrhage must have been very extensive. The wound was enlarged by a vertical incision of about four inches, and as soon as the abdomen was opened fresh hæmorrhage of an alarming character occurred; this was partially controlled by thrusting the left hand upwards against the liver, where a large irregular cavity was felt. With the right hand large masses of clotted blood were removed from the right kidney pouch and flank,

and the intestines were packed off with gauze swabs. It was then seen that there was a large hole, nearly the size of the closed fist, involving the anterior surface and sharp margin of the right lobe of the liver, with fissures extending from it in various directions; several pieces of liver up to a couple of inches in size were almost completely detached; indeed, it seemed as if almost the half of the right lobe had been burst and pulped so much so that it appeared quite hopeless to attempt closure with sutures. Iodoform gauze was therefore packed between the outer and anterior surface of the liver and the diaphragm and abdominal wall, and also under the liver all round the injured area, and then the cavity in the liver was packed tightly with iodoform gauze. By this means the hæmorrhage was checked, except for a very slight oozing. On one of the torn fragments of liver a vein as thick as a quill tooth-pick was exposed for nearly two inches, with most of its tributory branches torn. The patient's condition was so bad during the operation that a subcutaneous saline injection was started before the operation was completed, and over 1½ pints were injected under the skin of the left axilla and chest. The abdominal wall was closed with through-and-through silk-worm gut sutures and Michel's clips. After the operation, which took less than half-an-hour, the patient was left on the theatre table surrounded by hot-water bottles and with the head lowered for an hour before he was sent back to the ward, where the foot of the bed was raised. The following day the dressings, which were soaked through with blood and bile, were changed. Except for severe pain, which necessitated the use of morphia for the first four days, and the onset of a considerable degree of jaundice accompanied by free escape of bile from the wound, the patient did well, and the plugs were gradually withdrawn, and the last of them was finally removed on Thursday, May 4th, and the stitches were taken out on the 6th. The bowels were not moved till the fifth day, when castor oil was given; at no time did the temperature rise above 100°. The jaundice persisted for some time and the wound closed slowly, a large piece of liver substance coming away as a slough about ten days after the

operation. The patient finally left the hospital completely healed on June 12th, seven weeks after his admission. When I saw him shortly before last Christmas he appeared in normal health, except for a ventral hernia in the region of the scar, and a complaint of general weakness. He is now back at work. From the appearance of the wound in the back, which was evidently the wound of entrance, it is probable that it was caused by a ricochet bullet, and this explains the extensive injury done to the liver. The shot was probably fired from the direction of Beggar's Bush Barracks, at a range of between 200 and 300 yards.

Case 2.—Bridget S., aged 13, was admitted at 5 p.m. on Friday afternoon, April 28th, 1916. She had been shot about one hour previously, and it was reported that she had vomited some blood. There was a small wound in the anterior abdominal wall about two inches above and to the right of the umbilicus, and another wound in the left lumbar region behind. Her condition was serious, blanched, fast pulse and respiration and some dulness in either flank, with distension of the abdomen. A vertical median incision was made, and on opening the abdominal cavity it was found full of partially clotted blood, and fresh hæmorrhage continued freely. The stomach was first examined, and a wound was found involving both the greater curvature and the great omentum; this wound did not penetrate to the interior of the stomach, and was only bleeding slightly. Then a double perforation of the small intestine was found, also a perforation of the mesentery close to its root, from which the chief hæmorrhage appeared to be proceeding. There was also a tear of the descending colon not involving the lumen and a perforation of the descending mesocolon. All the perforations were closed and the hæmorrhage checked, most of the clots and fluid blood were cleared out, and the abdomen filled with hot saline fluid and the abdominal wall closed with through-and-through silkworm gut sutures and Michel's clips. Saline was also injected into the axilla during the concluding steps of the operation, which lasted forty minutes. When returned to the ward the

patient's condition was not too bad, but she died suddenly about midnight. Here we were unable to find out at what range the shot had been fired, but the wounds were probably produced by a modern service rifle.

Case 3.—A. B., a man of 46, was admitted to hospital on Saturday afternoon in a very collapsed condition. There were two abdominal wounds, a large one in the right nipple line one inch under the margin of the ribs, with everted omentum. A smaller wound was present in the left lateral wall of the abdomen. Mr. Johnston opened the abdomen through the right wound but found no serious damage in the neighbourhood; at the same I opened the abdomen through the left linea semilunaris and found an extensive perforation of the descending colon, which I closed; hæmorrhage was still proceeding from nearer the middle line, and it was then found that the small intestine close to the duodeno-jejunal flexure was extensively torn: it was completely divided about ten inches from the flexure, and completely divided again about 18 inches lower down, and between the two divisions there was an irregular longitudinal tear of nearly three inches in length. The portion between the complete divisions was resected and an end-to-end anastomosis effected; the mesentery tied off, the blood clots removed and the abdominal cavity filled with hot saline, and the incisions closed with through-and-through silk-worm gut sutures. The patient never rallied, and died about two hours after the operation. The injuries in this case were probably caused by either a ricochet or a blunt-nosed bullet, or else one of the large, soft-leaden sporting bullets so extensively used by the rebels, as the smaller of the two wounds, that in the left lateral abdominal wall, was considerably larger than would be caused by a modern bullet.

There were two other cases which may be included, though neither of them was operated on at the hospital.

Case 4.—This was an old man who was brought in by the Pembroke Ambulance on Sunday—he had been shot through

the abdomen from side to side about an hour previously. The wounds were both small, and situated the one in the right and the other the left lateral abdominal wall. When admitted his condition was so bad that it was not thought advisable to operate, and he died a few hours afterwards, without recovering from the condition of shock in which he was when first seen.

Case 5 was also an elderly man, who was admitted late one night. He had been shot in the right inguinal region, and there were no symptoms to determine the track of the missile which had lodged. No definite abdominal symptoms had developed by the following day, when he was removed from the hospital and operated on, I believe, elsewhere. We never heard what was found at the operation, and the only information given was that the patient died.

Though the general result of these cases was so disappointing, all except the first proving fatal, yet it must be admitted that they tend to confirm the teaching of the present day, that early operation is the only hope in cases of abdominal gunshot injuries. The first case, the rupture of the liver, was an unexpected success, for the condition of the patient before operation seemed almost hopeless, and even after the conclusion of the operation his chances of recovery appeared to be extremely small. Here operation, fortunately performed early, was undoubtedly life-saving. In the second case, the young girl with multiple injuries to stomach, small and large intestine, there seemed to be a chance of recovery after the operation, in spite of the severity of the injuries. Probably, if the patient had been older or more robust, or if the operation could have been done half an hour earlier, success might have crowned our efforts. In the third case, the man with complete double division of the small intestine, and

with injury to the large bowel also—the condition was probably hopeless, owing to the extensive nature of the injuries, and all that can be said is that he could not have lived more than an hour or two in any case. In the fourth case, that of the elderly man, his age and condition on admission precluded any hope of a successful operation, and in the last case, while the patient was under our charge, there were no definite indications of abdominal trouble, and when the question of exploratory operation was suggested it was at once refused. One lesson was deeply impressed upon me by these cases, namely, that in abdominal gunshot injuries hæmorrhage is of cardinal importance, and all one's efforts should be at once directed to its immediate control. While the abdomen is closed there is a positive intra-abdominal pressure which increases *pari passu* with the bleeding, and therefore to some extent controls and checks the hæmorrhage ; as soon, however, as the abdominal wall is incised this pressure disappears, and bleeding, which up till then has gradually lessened, and may perhaps have ceased, starts again with redoubled fury, and may prove most alarming. It is therefore most necessary for the operator, when he has discovered one injury to the viscera, not to waste time in repairing it before he has made certain that there are no other and more important injuries, and when he finds that the injuries are multiple that which is giving rise to the greatest hæmorrhage should be the first to receive attention. In some cases, where serious multiple injuries exist, time may be saved if the assistant of a second operator undertakes the repair of some of the lesions whilst the surgeon himself is repairing the others, and cases may even occur where, as in the third case reported, two abdominal operations through separate incisions may with

advantage be carried out at one and the same time. In this work, above all others, everything must be done to minimise the duration and shock of the operation, and also to try and counteract the shock from which the patient is already suffering, partly as the result of the injury and partly as the result of the consequent hæmorrhage.

FEVERS AMONGST THE TROOPS IN EGYPT.

By T. GILLMAN MOORHEAD, M.D. Dubl.; F.R.C.P.I.;
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[Read in the Section of Medicine, January 26, 1917.]

Owing to the fact that I have already made my period of work in Egypt the subject of a lecture at which some members of the Academy were present, I have had some considerable difficulty in preparing the present communication, in accordance with our generally recognised principle that nothing must be read at the Academy that has previously been communicated elsewhere. My lecture at the Royal College of Surgeons was of a semi-popular nature, and in consequence I have endeavoured now to deal almost entirely with the scientific side of the work, but for any overlapping that occurs I offer apologies.

In the present paper I have confined myself almost entirely to the febrile diseases that I encountered amongst the troops, as to deal with the medical aspects of war work in the East would involve too lengthy a paper. I hope, however, on a future occasion to deal with some general medical problems now purposely left unconsidered.

On January 1st of this year, after a pleasant voyage, I arrived in Alexandria, and on reporting to the A. D. M. S. was directed to proceed to the —— General Hospital, to which I was informed I had been already detailed in

London. I accordingly did so, and commenced work on the following day.

During the course of the following weeks I gradually acquired a knowledge of the general medical establishment in Egypt, an establishment which had to cope with the requirements of the large Army then in the country, and with a large percentage of the medical and surgical casualties from Gallipoli, and later with cases from Mesopotamia. As far as Alexandria was concerned I found that four large general hospitals existed for European troops, two hospitals for Indian troops, and that, in addition, the local Egyptian Government Hospital had been to a large extent placed at the service of the Military Authorities. Further, numerous convalescent homes existed—varying in size from one capable of receiving well over 1,000 patients, to smaller homes adapted for the use of from 30 to 40 officers. Of the four general hospitals my own was the largest, and in many ways the best situated. It was capable shortly after the beginning of this year of accommodating more than 2,500 patients, and was managed by a staff of forty medical officers. The buildings, three in number, which formed its nucleus, were known collectively as Victoria College, and owed their origin to the fact that some fifteen years ago the citizens of Alexandria, desiring to erect a memorial to Queen Victoria, decided that the best tribute they could pay to her memory would consist in founding and properly housing a school where a first-class European education could be obtained. Like most other European buildings in Egypt, the College was erected on a generous scale, as far as both internal and external space was concerned, the large buildings being surrounded by several acres of enclosed land. The College was situated about seven miles from the centre

of Alexandria, at the terminus of a suburban tram line, and was practically the last outpost of the suburbs of the city. Of the three buildings, one had been allocated entirely to surgical work, one to general medical work, and one to enteric cases, but only a small number of the patients, comparatively speaking, were treated within the buildings, and by far the larger number were dealt with in huts and tents, which, as far as possible, were grouped into special areas, according to the type of cases admitted : thus, there was a special compound for dysentery cases, one for ordinary infectious cases, one for pulmonary tuberculosis, and so on ; and finally, there was a large compound for venereal work.

My own work within the hospital consisted in superintending generally all the medical cases, and seeing any cases on the surgical side for which a medical opinion might be required. I generally started the day by visiting a ward with about 60 beds, which was established shortly after my arrival, and which was called the Observation Enteric Ward. Into this all cases of doubtful pyrexia were admitted, and from it were weeded out each day cases to their appropriate destination. This ward was to me more interesting than all the remainder of the hospital, as here one had to differentiate not merely the fevers with which one is familiar at home, but also malaria, relapsing fever, amœbic hepatitis, sand-fly fever, sunstroke, unusual dysenteries and the like.

The cases coming under the group term "enteric" were at first numerous, and of a rather severe type. We were, in fact, faced during January, February, and March with an extensive epidemic, mainly localised in one large camp. Analysis of these cases showed that 85 per cent. at least, and I think I may say later on 95 per cent., were

cases of paratyphoid. Cases due to the *B. typhosus* itself were rare, and any bad cases I saw were in uninoculated patients. Of the two varieties of paratyphoid, para A. was much more frequently met with than para B. Up to the end of March we were dealing for the most part with patients who had not received the T. A. B. vaccine; after March most patients admitted had had both typhoid vaccine independently and also either one or two doses of T. A. B.

As regards diagnosis, the rule was made that an immediate blood culture was to be performed on any case which was regarded as suspicious on preliminary examination. Five cubic centimetres of blood was withdrawn from a vein and inoculated into 2½ per cent. taurocholate solution. If any growth occurred it was plated out the next day, and usually within 48 hours an exact diagnosis was possible in those cases that afforded a positive blood culture. These, however, were not a very large proportion of the whole, and consequently other methods were required. Personally, if a blood culture proved sterile, I relied practically entirely on clinical evidence, but Widal tests were carried out systematically. In relation to these tests I soon concluded that in inoculated patients they were valueless. Many interesting details came to light, but the whole question of agglutinins is too complicated for me to deal with at present.

I may, however, point out that when a patient who has been inoculated against the *B. typhosus* becomes infected with a paratyphoid bacillus, the first apparent effect upon his blood is to increase enormously his latent agglutinins against the *B. typhosus*, and that it is only later on that the agglutinins against the infecting paratyphoid organism appear. This fact is generally admitted, but I soon con-

vinced myself that other pyrexias could similarly increase the anti-typhoid agglutinins, and therefore one was compelled at once to abandon the Widal test as far as the *B. typhosus* was concerned. Widal's for paratyphoid were fairly satisfactory, till one began to meet cases who had received the T. A. B. vaccine. In such cases the agglutinin test was of little use. I had a series of patients who had been admitted for non-pyrexial conditions, and who had had this vaccine tested, and found that practically all of them gave as pronounced a positive reaction as those patients who actually had paratyphoid fever. Some bacteriologists still maintain that if the test in done every second day and a rising titre is obtained to say para A. or B., a definite diagnosis is justifiable, but few patients are willing to submit to these frequent punctures, and few bacteriologists relish the performance of so many tests. Owing to the failure of the Widal test there is a disposition now to rely more on cultures from the urine or fæces, but although I was one of those who at first advocated these, I soon found that a clinical diagnosis of the group "enterica" could as a rule be made certainly long before the bacteriologist could arrive at a conclusion by these methods.

Of the clinical symptoms I have very little to say, as I have already published a paper on the subject. If asked to state briefly how paratyphoid differed from typhoid I would say that a sudden onset was much more common, and that as a rule the disease was milder and shorter. A common early symptom is pain and pronounced tenderness on pressure over the splenic region, while enlargement of the spleen occurs at a much earlier date than in typhoid and to a greater extent. Mentally, there is drowsiness and lethargy at first, but later on there is

usually much alertness of mind, so that on walking through a ward one is struck by the fact that several patients with a temperature of perhaps 103° F. or higher are reading books or chatting to their neighbours. Early severe frontal headache is present, but disappears in the second week. The pulse is even slower than in enteric, often being 60 per minute, or even less, at the height of the fever. Spots are usually late in appearing : they are often abundant, and are browner and larger than the rose spots of typhoid.

As already stated, differential diagnosis presents more problems in Egypt than at home owing to the prevalence of relapsing fever, malaria, &c.; but over and above these diseases one is also met with the fact that a palpable spleen seems to occur in almost any pyrexia, and is even met with in patients in whom no history of disease at all is obtained. The value of an enlarged spleen from a diagnostic point of view is in fact very slight. After the T. A. B. inoculation had become common, an interesting new disease made its appearance, and was for the first time recognised in a General Hospital. This was a pyrexia, apparently caused by the *B. fæcalis alkaligines*, another member of the enteric group of organisms. Altogether, up to my departure, 21 cases were met with in which this organism was isolated from the blood. The symptoms to which it gave rise resembled at first enteric, but splenic enlargement seldom took place, and after about five days the temperature usually fell to normal, to be followed, however, after an interval of three or four days by a second but shorter pyrexial period. Sometimes a third or even a fourth period of pyrexia occurred. The blood of patients from whom this organism was isolated developed contra-agglutinins, while other enteric cases would

not agglutinate the *B. fæcalis*. After our discovery of the condition the same organism was found in several other hospitals in Egypt, and the number of cases of P. U. O. began to diminish.

From the Army point of view paratyphoid presents many other interesting aspects, but I will only refer, and in the briefest terms, to two more. In the first place, I found that the mildness of the symptoms in many cases led to the use of purgatives rather freely, and to comparative abundant feeding : both are a mistake. Unwise treatment will readily convert a mild case into a serious one, and a very short experience should convince anyone that a very strict liquid diet and complete abstention from the use of purgatives during the pyrexial period is the only safe course to adopt. Vaccines I used extensively in those cases where the actual causative organism was identified. In doses of 100 million given early they appeared useful; occasionally they seemed to be followed by striking improvement; more often their effect was nil.

As regards carrier cases, it was felt from the beginning that very strict precautions should be taken. In consequence, all cases were at first sent home in the convalescent stage, and were thoroughly investigated before returning to duty. Later on, when equipment was available, we were able to undertake this work ourselves, and every case was subjected to a triple test of urine and fæces before being returned to his unit.

Relapsing fever, of which I saw a good deal, on admission resembles fairly closely paratyphoid with an acute onset, but as a rule can be distinguished by the general severity of symptoms. The face is usually flushed, intense frontal headache is present, there are often severe general pains with much restlessness, and from the first

the spleen may be much enlarged, though this last is by no means a constant symptom. In every case I saw, however, there was one feature that contrasted strongly with paratyphoid—namely, the rapidity of the pulse : so that the rule was even made that a blood film was to be examined in all cases where this symptom was noted. The spirochæte that produces the disease varies slightly in size and characters according to the locality, and also varies as regards its intermediate host. The Egyptian type, which is apparently identical with that found in Southern Europe, is almost certainly transmitted by lice, while in West Africa and the Congo the tick is the insect by which the spirochæte is conveyed to man. All varieties stain readily, though not deeply, by ordinary aniline dyes, dilute carbol fuchsin giving as good results as any. Leishmann's stain and Giemsa's also gave satisfactory results. When a certain diagnosis is made the best treatment consist of giving at once an intravenous dose of "606" or "914." I found that .3 gramme of either was quite sufficient to bring about not merely an immediate fall of the primary fever, but also to prevent subsequent relapse. A larger dose should not be given, and even with the doses I have named one must be prepared for a subsequent fairly severe collapse. I usually found that about five hours after the injection the patient became attacked with severe diarrhœa and vomiting, the pulse became fast and running in character, the extremities cold, and profuse perspiration took place. Coincident with all this, the temperature fell abruptly, perhaps from 104° F. or 105° F. to subnormal. After a couple of hours the symptoms of collapse pass off, the patient falls asleep, and next morning feels perfectly well. The result is, in fact, as dramatic as anything I have ever encountered as a result of treat-

ment. In my opinion the collapse is due not to the drug injected, but to the rapid destruction of spirillæ and the consequent liberation of toxins which is brought about. The first case I treated caused me considerable alarm, as none of the books or papers I have seen make the smallest reference to the severe reaction I have described, but I soon found that the patient responded readily to ordinary restorative measures, and, therefore, I came to look on this collapse as quite a normal occurrence. There were no complications and no fatalities amongst the cases we treated. At one time the supply of both "606" and "914" ran out, and I then tried the use of mercury and potassium iodide in big doses, but without effect: one of the cases treated in this way having no less than four relapses.

Next to paratyphoid the dysentery cases constituted the largest single group of admissions of any one disease. At first most of the cases were of the amœbic or mixed variety, but soon after February these began to fall off in number, as the cases from Gallipoli were gradually disposed of, and bacillary dysentery then made its appearance, and for several months numerous cases were admitted almost daily. After June some amœbic cases again appeared amongst troops on their way home from Mesopotamia, but as far as Egypt itself was concerned no recurrence took place.

The whole subject of dysentery is too big and complicated to permit of more than the most cursory reference. The name is essentially a clinical one, and is applied to a group of diseases characterised pathologically by inflammation of the mucous lining of the colon, and clinically by the symptoms of abdominal pain, tenesmus, and the passage of stools containing mucus, and frequently blood.

Two main varieties—namely, amœbic and bacillary—are recognised, but mixed cases also occur, and similar symptoms may be produced by protozoon parasites distinct from amœbæ.

The presence of amœbæ in diarrhœic stools was demonstrated by Lambl in 1859, but to Lösch belongs the credit of definitely suggesting their importance as the cause of dysentery in 1873, and of applying the term amœba coli to the protozoa he described. Soon after his description other workers found amœbæ in the motions of quite normal individuals, and doubt was thrown on the importance of Lösch's observations. Kartulis then undertook a research in the numerous dysenteric cases admitted to the Government Hospital, Alexandria, a hospital to which he was then and is still the pathologist, and not only demonstrated the presence of amœbæ in his cases, but also showed that dysentery could be produced in cats by the injection per rectum of stools containing amœbæ, and that amœbæ could be found in the mucus passed by these infected animals. Some doubt nevertheless continued in the minds of pathologists until Schaudinn published his researches in 1903. He definitely proved that the human intestine may be infected with two varieties of amœbæ—one harmless and the other pathogenic. For the former he retained the name amœba coli, while he christened the latter amœba histolytica, owing to its power of eating its way into the colonic mucous membrane. It is now universally admitted that the amœba histolytica is the cause of amœbic dysentery.

These two forms of amœbæ differ in certain characteristics, but are not easily distinguished in the freely mobile stage without much experience. Speaking generally, it may be said that an amœba presenting a fairly sharp dis-

tinction between its endoplasm and its ectoplasm, and containing within its red blood corpuscles, may be regarded as almost certainly the amœba histolytica. Both organisms under unfavourable circumstances readily form cysts, which are frequently passed in large numbers in the stools, and which, fortunately, are easily distinguishable. Thus, the amœba coli gives rise to circular, well-defined cysts, containing eight nuclei, while the amœba histolytica forms smaller, more refractile cysts of a slightly greenish colour when viewed by transmitted light, and never containing more than four nuclei. To this type of cyst is given the name amœba tetragena, and its presence in mucoid stools is positive proof of the existence of amœbic dysentery. Free amœbæ are very easily destroyed, but the cysts are fairly resistant, and it is through them that infection is spread.

As regards mode of transmission, it is almost certain that infection may be directly conveyed by vegetables, fruit and other articles of diet that have come directly contaminated by fæces; but probably one of the most striking medical discoveries made during the war has been the demonstration by Wenyon of the part played by the common house fly. Wenyon allowed flies to settle on fæces containing cysts, and on removing them found that within 20 to 30 minutes they began to pass droplets of liquid excreta in which cysts could be found with the greatest ease. Even as long as sixteen hours after being fed with the cyst containing material, cysts were still detectable in the excreta, so it is plain that flies that have a range of flight of about a mile can very readily act as transmitters of infection. This discovery of Wenyon's added impetus to the campaign against flies that had been waged in Egypt since last winter, and was probably

responsible for the great diminution in amœbic dysentery cases to which I have alluded.

It is plain that not only is it important to kill flies and to destroy their breeding places, but also to prevent the access of those that survive to food and any infected stool. Stringent precautions against this were taken everywhere throughout Egypt where troops were located, and special care was taken by burning and disinfecting horse manure to destroy fly larvæ and eggs; while, of course, the question of latrines and general disposal of human excreta was thought out according to the best means available in each district. This last precaution is most essential, as an examination of 1,000 apparently healthy men from Gallipoli revealed the presence of 80 carriers of pathogenic cysts.

When amœbæ do reach the colon they multiply rapidly, and making their way through the mucosa produce necrotic changes in the sub-mucosa, which leads to extensive irregular ulceration. This ulceration may involve the entire colon or may be confined to a small area. The symptoms that result may be acute or, even from their onset, chronic; and the acute form may vary in intensity from a quite mild disease to one with the most intense pain, almost continuous diarrhœa, and the passage of mucus, blood and gangrenous intestinal sloughs. The most common variety is one in which the patient has had mild diarrhœa for two or three days, and then begins to suffer from very frequent stools with tenesmus, and notices for the first time that blood and mucus are present. There is usually no rise of temperature, no nausea or sickness, and there is a pronounced leucocytosis. Physical examination is negative, except that one may detect the presence of spasm and thickening of the cæcum or of other parts of the colon.

The chronic cases include patients with persistent slight mucoid diarrhœa, and completely latent cases, in which cysts may only be found in the course of routine examination. It is almost certain, however, that even in this last-mentioned variety some small scattered ulcers exist.

The treatment of amœbic dysentery has been comparatively simple since the discovery by Rogers a few years ago of the specific action of emetine. Without going into full details, one may state at once that each patient should be given a course of 10 to 12 grains of emetine, administered in daily doses of one grain hypodermically. In spite of the fears expressed by some regarding the toxic action of this drug it may be stated dogmatically that these doses are quite safe. Only once during my entire stay did I meet with a case in which bad effects were apparently produced, and then only temporarily. Patients should be kept in bed during the treatment, and it is only right to add that when the drug is pressed, as it was in the early stages of the war, in a few cases up to 30 to 40 grains, nervous tremor, palpitation and anæmia may result. If a 12-grain course fails to destroy all cysts, ipecacuanha by the mouth or lavage of the colon with saline solution containing one grain of emetine to the pint is often useful. If a case is seen early, and a 12-grain course given, nothing more is as a rule required.

The remaining treatment is commonsense. Liquid diet, with at first no milk, hot applications to the abdomen, and a daily dose of some saline aperient are all required. If pain is severe a small dose of morphia may be required, but this should never be given unless absolutely necessary. Much care also is required in returning to solid diet.

Cases that have become chronic—as, unfortunately, many do when the disease starts in the trenches or else-

where, where systematic early treatment cannot be carried out—may strain one's resources to the utmost, but into the details of such cases it is now impossible to go. Many of them become secondarily infected with bacilli, and hence the difficulty of cure is increased.

The complications of amœbic dysentery that I encountered were three in number—namely, (1) amœbic liver abscess and amœbic hepatitis, (2) amœbic arthritis, and (3) post-dysenteric meteorism.

Liver abscess was rare, though I saw one case in which the abscess was as large as a football, and formed an enormous swelling, filling up the whole of the subcostal region. Amœbic hepatitis was more common, and by no means easy to diagnose. Several cases were admitted as suspected enteric, and I saw them in the surgical wards, where they had perhaps been for a fortnight or more suffering from obscure pyrexia. Physical examination would perhaps show some enlargement and tenderness of the liver, but as this is far from uncommon in paratyphoid, one could not exclude this disease till a negative blood culture was returned, and other tests proved negative. As a rule, a history of previous dysentery was obtained, but the final test depended on the use of emetine. When this drug is given the temperature falls at once, the patient at once feels better, and after perhaps weeks of listlessness and anorexia, begins to clamour for food, and to be allowed to get up. It is of course an axiom in tropical countries that such cases may be mistaken for paratyphoid, but I confess until I had encountered the cases myself I was sceptical as to their existence.

2. Concerning amœbic arthritis I have already published a short article, and I need only say here that I met with several cases of multiple arthritis resembling gonorrhœal

arthritis which failed to clear up on any treatment till their true nature was recognised. A course of emetine brought about in these cases as rapid a cure as that I have already referred to in cases of amœbic hepatitis.

3. Post-dysenteric meteorism. Quite a large number of patients were admitted complaining of abdominal distension, of flatulence and general digestive disturbances, which had set in gradually a month or so after returning to duty convalescent from severe dysentery. On examination the abdominal distension was often enormous, but there was no constipation, and apart from the abdominal condition nothing could be made out on examination. Bismuth meal examination threw no light on the condition, and revealed no undue stasis, and treatment was quite unavailing, though every available remedy, including massage, &c., was tried. What the exact nature of these cases is I am at a loss to understand. I thought at first that stenosis might exist somewhere in the colon, but the *x*-ray examination negatived this idea, and I can now only suppose that it was due to general intestinal atony. As far as I am aware, nobody has previously described this condition, and at first I failed on enquiry to hear of similar cases in other hospitals. Later on, however, Col. Lister, of the —— General Hospital, told me that he also had seen several such cases, and had also completely failed to benefit them. All the cases I saw were quite unfit for duty, and all ultimately were invalided home.

Of bacillary dysentery we had even more than of amœbic, and to fully describe its characteristics and varieties, of which we recognised at least six clinical forms, would occupy an entire paper. I will, therefore, only deal with it in the briefest possible manner.

Numerous allied bacilli are now known to be capable of

causing it, but the two best known are the shiga and flexner. These are mainly distinguished from one another by the fact that the latter ferments mannite and the former does not. From symptoms alone we found it quite impossible to say which baccillus was likely to be present, but more than 80 per cent. of the organisms actually isolated were of the flexner type.

The inflammation produced is primarily one of the mucous membrane, and is usually extensive. As a rule it rapidly leads to superficial ulceration of the entire colonic mucosa. It differs from amœbic colitis in that the amœbic is essentially a disease of the submucosa, and isolated ulcers appear with healthy areas of mucous membrane between them, whereas in bacillary the involvement is from the surface, and healthy areas are not seen between the lesions. In bacillary also the lower part of the ileum may be involved in the inflammation.

Clinically the symptoms usually set in suddenly with severe pain, colic, tenesmus, diarrhœa, with blood-stained mucus and rise of temperature. There are often marked toxic symptoms with rapid pulse and dry tongue, but there is no leucocytosis. All varieties, from very mild to very severe cases, occur.

The most acute cases of all resemble cholera, with profuse rice-water stools at first, the blood and mucus only appearing 24 to 48 hours after the onset. There are cramps in the legs and arms, the pulse becomes uncountable, the face is shrunken, blood pressure falls, and there are all the phenomena of extreme collapse, so that within a few hours from the beginning of the attack the patient may be almost moribund. Such cases, though they only formed a small percentage of the total admissions, were sufficiently numerous to require special arrangements to be made for their immediate treatment on arrival.

The treatment of bacillary dysentery is much the same as that for amœbic, except that instead of emetine one uses polyvalent anti-dysenteric serum. The usually recommended doses of this serum are much too small: most books suggest 10 to 20 c.c., but in an average case one should never give less than 60 c.c., and this should be repeated in 24 hours unless there is a very marked improvement. A bad case should receive an immediate dose of 100 c.c., repeated each day or every second day, until 400 c.c. have been given. In the very acute cases that I have described the serum must be given intravenously, and may be added to a couple of pints of hypertonic salt solution, that should be administered by the intravenous route at the earliest possible moment.

Provided a sufficient dose of serum is given, one expects to get rid of the blood and mucus in an average bacillary case in about a week. A few cases, however, run on much longer, and in those I found the best results were obtained by waiting for about a fortnight after giving 400 c.c serum and then starting again. Under such circumstances one may encounter severe anaphylactic reactions, but in the very serious kind of case that is in my mind this risk must be taken.

Lavage of the colon in subacute and chronic cases with copper sulphate solution or silver nitrate is often distinctly helpful, and, of course, careful nursing is all-important. In all cases a preliminary big dose of castor oil must be given, and later on salines, as in amœbic dysentery.

If one should be called on to treat an acute dysentery under circumstances where it is impossible to distinguish between amœbic and bacillary, the undoubted proper course to pursue is to give both serum and emetine.

Neither will do any harm, and if one merely administers one and hopes for the best, the patient may die or drift into a chronic stage before one has an opportunity of using the other, if the wrong remedy has been chosen.

Bacillary dysentery, as I saw it, had practically no complications. It leaves, however, prolonged debility, often with some cardiac dilatation.

In addition to dysentery and enteric many other diarrhœal conditions were prevalent in Egypt. Indeed, as might be expected, gastro-intestinal troubles of one sort or another formed about one-half of all the medical illness encountered. Among the most interesting of the chronic non-ulcerative diarrhœas is that due to Lamblia intestinalis, a flagellate easily recognised in the fæces either in its free or encysted form. It apparently produces irritation of the small intestine and colon, which results in the passage of frequent, often bright yellow, fermenting stools. In a few cases also mucus may be present. The best treatment consists in putting the patient to bed for a few days on liquid or semi-liquid diet and giving beta-napthol in large doses.

Along with intestinal troubles we may classify jaundice, which was very widespread in Gallipoli, but of which I only saw about 70 cases. The whole subject of jaundice is at present much confused. A great many cases that occur in the war regions appear to be ordinary catarrhal jaundice, and some cases are undoubtedly the result of known infections, such as paratyphoid; where, however, the line is to be drawn between the ordinary catarrhal variety and Weils disease is the difficulty, and when the term Weils disease is to be dropped and the term Mediterranean yellow fever used, is a still further difficulty. Some, indeed, use the terms Weils disease and Mediter-

ranean yellow fever as synonymous terms, and introduce the name camp jaundice as an extra term to connote the type of case which symptomatically intervenes between the catarrhal and the severe hæmorrhagic or yellow fever type.

Camp jaundice as thus defined begins with three to four days' sharp fever, accompanied by general malaria, nausea and rapid pulse. About the fifth day jaundice appears, and gradually deepens; the liver becomes enlarged and tender; the spleen also is big, and a relative lymphocytosis develops. In some cases severe cardiac dilatation and symptoms indicative of myocardial poisoning are present. The jaundice usually lasts two or three weeks, and during convalescence there may be much debility. In several cases of mine blood and urine cultures were made, but all were sterile. Spirochætes also were looked for, but again with negative results. We, however, had none of the severe hæmorrhagic cases in which spirochætes have been demonstrated by Inada and Ito in Japan, and by Adrian Stokes in Flanders.

There is a general agreement that whatever the bacterial cause of camp jaundice may be its occurrence is predisposed to by insanitary camp conditions, and by the consumption of spoiled or foul food; and at present it seems certain that preventive measures directed towards the improvement of camp sanitation is the only way in which we can hope to check the heavy incidence of the disease in trench warfare.

The other febrile conditions with which we had to deal do not require more than a few words, and I will only refer to a few special points which I think are of general interest.

In the first place, heat stroke is important, not only

from its immediate effects, but also from the subsequent ill-health to which it gives rise to, and as far as I can determine it is a condition that has never been properly investigated. In using the term heat stroke I deliberately exclude heat exhaustion, which is much less serious, and refer only to those cases which start with severe headache, vomiting, great restlessness, cutaneous hyperæsthesia, and hyperpyrexia. In the worst cases of this kind coma and paralytic symptoms may rapidly develop; but what interested me most was the general hæmorrhagic tendency that these patients exhibited: thus, bleeding from the kidney and from the bowel may occur, and hæmorrhagic exudation seems frequently to occur into the cerebro-spinal fluid. This last fact we discovered by doing punctures on all bad cases, and finding the fluid in each case definitely stained with blood. Culture of the fluid was in each case sterile. From a therapeutic point of view, we found that puncture gives relief, often immediate and permanent, to the headache, and also relieves the restlessness of the patient. As far as I am aware lumbar puncture has not been advocated previously as a treatment for sunstroke, but my own limited experience leads me to urge that it should always in future be employed in bad cases, more especially those of a comatose or paralytic nature.

Cases coming back from Mesopotamia after suffering from sunstroke presented a special difficulty. Many of them looked perfectly well, but they all, without exception, complained of severe headache on the slightest exposure to the rays of the sun. Careful examination usually revealed nothing wrong, and one was tempted to think that the strong desire to return to "Blighty" led in many of them to definite malingering. If, however, a man complains of headache, refuses to work, and has a

definite cause to which he can ascribe the onset of his alleged trouble, one is helpless, and therefore all these cases were invalided home. I am bound to add that a few keen officers made similar complaints, so possibly all the men were quite genuinely unfit; but if any member of the Academy can tell me how to distinguish between a real and an assumed headache under such circumstances I will be more than obliged.

Cerebro-spinal fever is a disease that everyone is on the look-out for in camps and barracks. We had a few cases, all of whom recovered after intra-spinal injections of Mulford's anti-meningococci serum. These cases I do not want to discuss, but in connection with this disease I came across what to me was a new condition. One day I was asked to see a man in the Surgical Division who had been admitted for suspected tetanus. He was given an intrathecal dose of anti-tetanic serum, and seemed at first to improve. Then other symptoms appeared, and hence my invitation to examine him. I found him lying with marked retraction of the head, and extreme stiffness of the neck and back muscles; the pulse was low, pupils unequal, respirations deep and there was a very pronounced Kernig's sign. In fact, but that there was some trismus, he was a typical example of cerebro-spinal meningitis, and the question was raised as to whether he could have been a meningitis case from the beginning. On puncture, a turbid fluid was withdrawn full of leuocytes, but sterile on culture, and containing no meningococci visible in a direct smear. The solution to the question raised came later on in the appearance of a short paper in the R. A. M. C. Journal, which described the condition met with in my patient as a frequent sequel of intra-spinal injections of anti-tetanic serum. The meningitis is sup-

posed to be the result of a chemical irritation, and in each case seen by the writer of the article the fluid had been sterile, so that no question of infection during injection arose. One other similar case came under my notice later on, and though I may be wrong in regarding the condition as unusual, it puzzled me so much at the time that I think it worth alluding to.

The next point I wish to refer to is the very satisfactory results we had with a stock pneumococcic vaccine in pneumonia. During April, May and June we had a small epidemic of a severe type, and high mortality. After exhausting every other method of treatment we determined, in despair, to try vaccine, although my slight previous experience with small doses and my *à priori* reasoning was against their use. We accordingly started with 50 million pneumococci on the day the diagnosis was made, and whether post or propter immediately began to get better results. We had no further deaths, and we began to get the crisis on the third or fourth day in almost every case. So impressed were the two men in immediate charge of the cases with the change for the better in our returns that they drew up a special memorandum with full reports for publication. These results may have been pure coincidence, and my mind is still open, but I feel bound to report what we found, in case it may be of use to others.

The last fever I will refer to is phlebotomus, or sand-fly fever, of which we had a small epidemic occurring in a unit that had been sent to a holiday camp near Alexandria from a desert station. Most of the men fell sick on the journey up or on the day after reaching Alexandria. The illness is like acute influenza, but is readily recognised by the presence of intense conjunctivitis, which gives the eye

the appearance sometimes seen in bloodhounds. Hence the local name of "dog disease" for this fever. Another characteristic is the presence of a marked leucopenia, the W. B. C. rarely exceeding 6,000 in number throughout. The disease is never fatal, and the acute pains which characterise it are readily relieved by a small dose of opium. In many parts of Egypt it was so epidemic as to tax the medical equipment severely, and to seriously deplete for the time being the strength of the units affected.

Outside the subject of fevers, with which alone I have dealt this evening, many questions of medical interest and importance arose, but I feel that I could better deal with them on another occasion. The problems presented by cardiac affection are at present exciting comment throughout the entire war zone, and were as numerous and important in Egypt as elsewhere. Venereal diseases also presented a special problem, from the point of view of prevention as well as from a therapeutic standpoint; while neuroses, malingering, and various skin diseases were also abundant, and of sufficient importance to warrant special consideration.

CONTINUOUS SPONGES.

By SIR WILLIAM J. SMYLY, M.D., F.R.C.P.I.

[Read in the Section of Obstetrics, February 2, 1917.]

Many a good thing is overlooked for want of advertisement, and I think that continuous sponges are an example of this fact. They were first introduced by Dr. Harry Sturgeon Crossen, and a very full account of them was published by him in the *American Journal of Obstetrics*, Vol. 59, 1909, and also in his book on Operative Gynæcology, 1915. His object was to devise some means by which the possibility of a sponge being left in the abdominal cavity during an abdominal operation might be avoided; and he claims that by the use of continuous sponges that object is attained; and, further, that it is the only method in existence which does it. In both statements I entirely agree with him, but I think it advisable to quote his own words:—"The difficulty of guarding absolutely against leaving a sponge in the abdomen is such that entire security against this fatal accident is counted one of the unsolved problems of abdominal work. Practically all writers on the subject state that there is no guarantee against its occurrence, even in routine hospital work, and with all the rules of co-operation and the special apparatus designed to prevent it." Neugebaur, in a most exhaustive consideration of the subject, comes to the conclusion that the accident is, to a certain extent, unavoidable. Schadner, in an excellent paper, states—"So long as surgery continues an art, just

so long will foreign bodies continue to be unintentionally left in the abdominal cavity." Findley states—"In former years the abdominal surgeon was seriously disturbed by well-grounded fears of secondary hæmorrhage and sepsis, but surgery has mastered these problems to a large degree, and they are little feared and seldom experienced. Now it is the thoughts of the sponge that disturb the night's repose when the report comes that something has gone wrong with our patient. The operator never can rid himself of the feeling of uncertainty as to the possibility of leaving a sponge." This expresses very well the feeling of those who have given attention to this subject and particularly of those who have personally experienced the accident and have been brought face to face with a concrete exemplification of the inadequacy of the usual methods.

The continued occurrence of this fatal accident from the failure of the preventive methods in general use constitute sufficient reason for calling attention to a method which the author has used with much satisfaction for several years. This method gives entire security, and at the same time is simple and inexpensive, and is effective in all conditions of abdominal work—in the emergency operation in the country with unfamiliar assistants, as well as in the routine hospital work. The failure of the safety methods in general use is due to their dependence upon sustained attention concerning the sponges, for it is required elsewhere. A method to be effective under all circumstances must be practically automatic, insuring the removal of all gauze without particular attention on the part of anyone at the time of the operation.

The method which, so far as I know, is in general use in Dublin, is to have a large number of small pads formed

of several layers of gauze sewed together at the edges and a few larger pads to keep back and protect the intestines, the small pads being tied together in bundles of ten before being placed in the steriliser. At the operation a special nurse looks after the sponges, and either collects them in bundles of ten, or, as in the Rotunda Hospital, hangs each sponge upon a hook on a special rack, which is formed of horizontal laths placed one over the other, and having ten hooks on each lath. At the end of the operation, but before the abdominal wound is closed, the sponges are carefully counted by the nurse and theatre sister, and the result is reported to the operator. In this method an error in the count is possible both before the sponges are placed in the steriliser and also at the time of the operation; and, as a matter of fact, I have had the misfortune to meet with this accident twice within recent years. In the first case the nurse reported that a sponge was missing, and a general search ensued. After a time I said to the nurse, "Are you right now?" and she said, "Yes, I am right." So I closed the wound and sent the patient to her bed. Before leaving the hospital, however, I asked the nurse where she had found the sponge. "I didn't find it," she said. "Then why did you tell me you were right?" "I told you," she said, "that a sponge was missing, and I was right." I had the patient brought down again, and found the sponge in her abdomen. The second case was a Wertheim's hysterectomy for cancer of the uterus complicated by a resection of the colon, a very complicated and tedious operation, in which a great number of sponges were employed, just the kind of case in which an accident of the kind was likely to occur. Three weeks afterwards there was a profuse discharge of pus through the abdominal wound, and a sponge was extracted; for-

tunately, the patient recovered. The only other plan which I have tried was having tapes attached to the sponges and forceps clipped on the tapes, but I soon abandoned it, because it rather increased than diminished my anxieties. At some operations which I had the advantage of witnessing in London I observed that generally the number of sponges was limited to a dozen, which when soiled were washed by a nurse and used again, a good plan as regards the special risk we are now considering, but seemed to me to be faulty with regard to sepsis. Many other methods are recorded in Dr. Crossen's papers, but none of them superior to that described above, which I have shown to be imperfect.

Having now employed Dr. Crossen's method of continuous sponges for some months, and being well satisfied with the results, I shall describe it to you.

The principle of this method is that no detached piece of gauze shall enter the abdominal cavity. Each piece of gauze introduced for sponging is simply part of a very long piece, the greater part of which is always outside the cavity. As used thus it consists of the following :—Four narrow strips, 10 yards long, 3 inches wide, 6 thicknesses. One wide strip, 5 yards long, 9 inches wide, 4 thicknesses. A similar set in reserve.

For the narrow strips the yard width of gauze is divided into two strips, and each of these, when folded to six thicknesses, is about three inches wide. For the wide strip the full yard width of gauze is used—when folded to four thicknesses it is nine inches wide. When folding, the edges are, of course, turned in, so that no ravelling can be left in the abdomen.

Each narrow strip is packed into a separate bag, 5 inches wide and 10 inches deep. The end of the strip first intro-

duced to the bottom of the bag should be fastened there securely by stitching through and through. Then pack the strip firmly into the bag, in such a way that it will come out easily, a little at a time as needed. Four of these filled bags belong to each set.

For holding the wide strip use a bag 6 by 10 inches, and open at the side instead of at the end. Fold the strip back and forth, thus forming a flattened pile about 5 inches wide. Fasten one end of the strip securely to the bottom of the bag by sewing through and through. Then place the folded strip in the bag in such a way that, when pulled upon, it will come out a little at a time, as a wide strip suitable for packing back the intestines. One wide strip and four narrow strips constitute one set, and are to be wrapped together in a cloth for sterilisation in the usual way. A second sterilised set should be held in reserve.

Method of using the Sponges.—Just before the incision is made a bag containing a narrow strip is fastened at the side of the abdomen by pinning it to the sterile sheet. The mouth of the bag lies conveniently near the wound, but not in the way. The end of the gauze strip is pulled out, as needed for sponging. In a case where but little sponging is required, one bag will be sufficient. In a case where more sponging is likely to be required, it is well to fasten a bag on each side of the abdomen at the beginning of the operation; this gives a sponge immediately at hand for both operator and the first assistant. As fresh portions of the strip are drawn out for use the soiled portions are dropped down beside the bag, and off the table. Troublesome accumulation of folds of the strip about the wound may be prevented by always dropping the soiled portion outside the field close to the bag. The bag containing the wide strip wrung out of hot saline solution, if required, is

laid on the abdomen above the wound and secured by two safety pins. The disposing of the soiled end of the sponge is the only difficulty I have met with in using these sponges. An inexperienced assistant will accumulate numerous folds around the wound and instruments and his own arms until he resembles the statute of Laocöon contending with the serpents. He will, however, soon learn to drop it out of the way. A more serious objection, however, especially in septic cases, is that it is liable to soil his sterilised overall. This difficulty, however, is obviated by a device introduced by Wakefield (*American Journal of Obstetrics*, October, 1912), and modified by Crossen. It is to have a large bag made of heavy duck and divided into two compartments fastened to the sterilised sheet. In one compartment is placed the bag containing the continuous sponge, and into the other are thrust the soiled portions as used. I use batiste for these bags.

Practice has convinced me that these sponges are a very great improvement, and are in no case inferior to separate ones; and they have absolutely abolished all risk of leaving sponges in the abdomen. They have, besides, some minor advantages which are worth considering. The nurses are saved an immense amount of time and labour which the cutting out and stitching of the separate pads involved, and which I have often remarked when visiting hospitals in the afternoon or evening, and have seen nurses seated at sewing machines making them. And the theatre sister in one of our general hospitals told me that it took on an average two hours to prepare the sponges for each operation. A continuous sponge can be cut, folded and placed in its bag in four minutes; and they save the worry and anxiety of counting them, and therefore they are most

popular with the nurses. Further, they are economical, because not only is the special nurse hitherto employed to count the separate pads eliminated, but the continuous sponges and bags are so easily washed and sterilised that they can be used again several times.

In vaginal work, too, where the operator has to do much of the sponging himself, I have found that, by attaching the bag to the sterilised sheet in the left inguinal region, with its mouth hanging over the fold of the groin, the strip when drawn out hangs in a most convenient position, always at hand when wanted.

In conclusion, I would repeat that, having used these sponges for several months, I am thoroughly convinced that they are the best in all and every case, and am surprised that they have not yet come into general use, and for that reason I have thought it advisable to introduce them to your notice.

Dr. Neill suggested that the sponges might be contained in pockets attached to the Kocher's sheet.

Dr. Madill said that, while he saw the good points of the routine, he had been unfortunate on the only occasion in which he had used the sponges. The case was one of pus tubes, where a great amount of sponging was required, and in manipulating the bag the entire sponge tumbled out; this he attributed to the want of practice of his assistant.

Dr. Alfred Smith was quite familiar with the literature on the uses and the advantages of the continuous sponges claimed by American operators. He saw the good points in the technique, and would try it should opportunity arise. He was satisfied with the security given by the "check" adopted in the Gynæcological Department of St. Vincent's Hospital, where the operator controlled the entire "team." Sponges in bundles of ten are counted under his

observation, in an audible tone, from the bundle to the receiving basin. Before closing the abdomen all sponges are again counted, and hung upon a wooden horse provided for the purpose.

Dr. Bethel Solomons said that while he had never had any mishaps with the single-sponge method, having seen the continuous sponges in operations under Sir William Smyly's guidance, he was extraordinarily struck by the benefits of the technique. He saw the slight disadvantages that might be present when working in different private homes or with different assistants. He thought that in minor laparotomies they might be extravagant, but the advantages were so much greater than the disadvantages that he intended to commence using them immediately.

Dr. R. White considered that there would be difficulty in teaching the assistant to use the sponges correctly, and that there was a great danger of infecting the unused portion.

Dr. Sheill remarked that it would be a simple matter to so design the bag and to so fold the continuous sponge that it would be quite impossible to accidentally pluck out the whole of it at one time. He suggested a bag triangular in section with the apex as the opening.

The President said there was always the fallibility of a count, and in difficult pus cases, where sponges are used rapidly, the risk is greatest, and the count cannot be checked by the operator. He (the President) had tried the continuous sponge in one case, which, unfortunately, was a difficult one, and he found the method inconvenient at first, but quite recognised that the trouble was due to the fact that it was a trial. He thought the continuous sponge was an advance in operation methods, and was the way in which the risk of leaving sponges in the abdomen could be completely guarded against. He thought, therefore, that it should be given a prolonged trial.

BONE GRAFTING FOR POTT'S CARIES.

By MAJOR W. I. de COURCY WHEELER, F.R.C.S.I ;

Surgeon to Mercer's Hospital, Dublin;
Honorary Surgeon to the Forces in Ireland.

[Read in the Section of Surgery, February 16, 1917.]

In dealing with the treatment of tuberculous disease of the spine some considerations of a general kind deserve attention. In the first place if the patient is a child, reasoning on the analogy of other tuberculous joints, we may hope for bony consolidation of the diseased vertebræ after a reasonable period of recumbency on a suitable apparatus. In the case of an adult we must assume that the tuberculous process is more likely to extend or remain stationary than to diminish and disappear. With a patient from the leisured class, either adult or child, more satisfactory result is likely to be obtained than in the case of a wage earner who finds it impossible to pay attention to treatment after discharge from hospital.

Again, it is possible to obtain results in special institutions suitably situated in the country, such as cannot be hoped for in city general hospitals, where patients can be maintained only under conditions unsuitable for tuberculosis and for much too short a time. It is for these reasons that a new operative form of treatment has proved so useful, a treatment particularly suitable for adult patients presenting themselves at general hospitals with

limited bed accommodation, and designed to cut short the old period of treatment by many months.

It was after 135 years of conservative surgery that operative treatment was first suggested, and it was not unnatural that surgeons were at first shy in adopting the newer methods. Before dismissing the subject of conservative treatment it is right to emphasise that there is no successful ambulatory method, and that the patient must be maintained in the recumbent position in a Phelp's box, a Whitman's splint, a Bradford or double Thomas frame, or such like appliance for in or about a year. Plaster, leather and poroplastic jackets should be reserved for the after-treatment to prevent recurrence.

In considering the operative treatment of Pott's caries it is hardly necessary to point out that the bone graft *per se* has no curative effect, but the immobilisation afforded by its introduction into the diseased segment of the spine is far more effectual than can be obtained by any form of apparatus. Albee thinks that fairly solid incorporation of the graft with the vertebræ takes place in about four weeks. Since bone grafting for various osseous lesions has become an every day operation in connection with war surgery, various theories as to the behaviour of the transplant attract attention. Some hold that the transplant dies and is absorbed and is replaced by the activity of the periosteum and endosteum, which alone survive. MacEwen's researches tend on the other hand to attribute the formation of new bone to the proliferation of osteoblasts within the bone itself, quite independently of the periosteum. Others again believe that the graft is only osteo-conductive and acts as a scaffolding about which the neighbouring bone-forming elements form a new graft.

In considering these theories it is interesting to note

how a graft actually behaves under certain pathological conditions. For example, in the presence of infection we are all familiar with the fact that detached portions of bone lying in a bath of pus in a compound comminuted fracture will survive and incorporate themselves with the callus in making a firm bony union. A transplanted graft will similarly survive infection if the infection is not too severe. In one of my cases a chronic osteomyelitis was established in the transplant as the result of infection from a bed sore. Twelve months after operation, when complete consolidation had taken place, the cortical part of the lower third of the graft was extruded as a sequestrum.

Again, if a bone graft is fractured accidentally it will unite by the formation of callus, and finally a graft will grow and survive in the abdominal wall with or without periosteum. We may therefore conclude that a graft is really viable and behaves to all intents and purposes exactly as it did when it was part and parcel of the bone from which it was removed.

My experience is limited to six cases; three have been operated on well over a year. One man had tuberculous periostitis and psoas abscess. He is back at work, has put on weight and is in every respect healthy.

The second case, already mentioned, was handicapped by osteomyelitis of the graft. She has now firm consolidation of the diseased spinal segment and is to all intents and purposes well.

The third case, that of a child with marked curvature, and the disease still active, was operated upon two years ago. In this case a rib was used as a graft in order that the curve of the transplant might accommodate itself to the kyphosis. When last seen the child was well.

In a recent case, which I show to-night, the patient is

about to leave hospital after three months' recumbency. The graft is seen in position in the photographs. The symptoms have disappeared, and the prospects are I think good.

One case of acute tuberculous caries of the spine, treated by rib grafting, died of acute acidosis a few days after operation.

In one adult I performed Hibbs' operation with an admirable result. In Hibbs' operation the base of the spinous process is fractured, denuded, and turned down into the space caused by the fracture, and turning down of the spinous process below.

Hundreds of cases of the treatment of Pott's caries by bone grafting have been published and the results leave nothing to be desired. Enthusiasts like Albee think that the bone graft is indicated in all cases and at all ages, and is even more necessary in the presence of psoas abscess or paraplegia. He and 33 other surgeons gave a report recently of 539 cases, in 460 of which the disease was arrested and in only 20 was no improvement recorded. The mortality was very low. Three died of tuberculous meningitis, and the other deaths, such as one from acidosis, could not be directly attributed to the particular operation.

In order to ensure success the following points should be observed :—There must be complete hæmostasis in the bed in which the graft is to lie. If there is much blood clot around the graft no granulations or capillaries can enter until the blood clot is organised. The delay may spell death to the graft.

Care of asepsis should of course be scrupulous, Lane's technique being adopted as far as possible.

The bed for the graft must be freshened to promote rapid adhesion and organisation. Immobility after operation must be secured.

There appear to be three good methods of applying a graft in tuberculous disease of the spine :—

Albee's method of splitting the spinous processes in the form of a V and inlaying the graft is well known.

Jones's method of applying the graft alongside the spinous processes, after freshening the bony bed underneath the erector spinal muscle, is simple, and has the advantage of giving an ample muscular covering to the graft.

Halstead divides the spinous processes at their base and lifts them up with the soft tissues. He then places the graft in position and allows the spinous processes back into place in such a way that the graft lies between the two divided bony surfaces.

In my first experience I was embarrassed by turning the patient over on his back in order to obtain the graft from the tibia after the spinal bed was prepared, the patient being again turned on his face and to put the graft in position. Such a procedure is, of course, necessary. By acutely flexing the knee on the thigh the graft can be obtained and the operation completed while the patient is prone.

In the absence of the electrically-driven twin saw, which is incomparably the best instrument for bone grafting operations, the graft from the tibia can be obtained by connecting a number of drill holes the entire length of the graft with touches of a chisel. Whatever the functions of periosteum the best results will be obtained if a graft contains all the bony elements, including periosteum, bone, and endosteum.

In conclusion, a warning issued by a recent writer is worthy of note. He compares the spinal column to a watch chain embedded in paraffin, the paraffin representing the muscles and other supporting structures. Under an

anæsthetic the spinal watch chain hangs in melted paraffin and can be twisted and buckled by any rough handling. In one case a push on the shoulder by the anæsthetist, in order to place the patient at a lower level on table, caused a complete buckle backwards of the tuberculous spine, which was reduced by manipulation. The result was, however, fatal.

As regards treatment after operation, a double Thomas's frame or Jones's abduction frame should be placed in position while the patient is still on his face. Frame and patient are turned together before removal from the operating table.

Personally I have kept the patients for about eight weeks on a frame, and after about three months in hospital allow them to return home with a simple spinal support.

Many surgeons, including Albee, deprecate the use of spinal supports following operation, and the results appear to be equally good. It is better, however, to err on the safe side.

ENTERIC FEVER WITH HYPERPYREXIA.

By HENRY C. DRURY, M.D. Dubl.; F.R.C.P.I.;
Physician to Sir Patrick Dun's Hospital, Dublin.

[Read in the Section of Medicine, February 23, 1917.]

J. R., aged 26, was admitted to Sir Patrick Dun's Hospital on Tuesday afternoon, February 6th, 1917. He stated that he had had a cold for the past fortnight, but he had felt worse since Thursday, February 1st, which appeared to be the starting point of the present illness. He had taken purgative medicines on Sunday, 4th inst., which had produced violent diarrhœa. This had subsided. He was sent to us as suffering from acute nephritis.

On examination there was nothing to be made out wrong except that the temperature was 101°, pulse 104; face flushed, and a small quantity of albumen in the urine.

He was quietly delirious at times during the night and appeared very ill, temperature having gone up to 103·8°.

Wednesday (7th) morning, temperature 103.8°, pulse 116. Careful examination failed to reveal anything further than a very few rhonchi here and there in the lungs, a dicrotic pulse, a full but not distended abdomen, and two or three doubtful spots on the abdomen; these were marked. That evening the temperature rose to 104.5°, and delirium was more marked, but of quiet type.

Thursday (8th). Temperature 103°, pulse 104, respiration 106. Quiet delirium at times. Conditions otherwise the same. Sore throat complained of, but nothing abnormal seen on examination. Two or three fresh spots

found on the abdomen and marked. Those seen the day before, fading.

Friday (9th). Delirium more marked and more energetic. Temperature 104°, pulse 120, respiration 28. A fresh crop of rose spots found and marked. The atropine tests for enteric were made, the pulse rate taken every ten minutes for two and a half hours, special note being taken from 20-30 minutes after the injection. The result was regarded as positive—viz., atropine $\frac{1}{33}$ grain given at 1.15 o'clock, pulse at 1 o'clock being 104; at 1.15, 104; 1.25, 111; 1.35, 110; 1.45, 108; 1.55, 110; 2, 106; 2.15, 104; 2.25, 116; 2.35, 116; 2.45, 120; 2.55, 112; 3.5, 112; 3.15, 112; 3.25, 112; 3.35, 112; 3.45, 112; 3.55, 112. The injection did not appear to have any ill effect on the patient, or in fact, any effect whatever. At night, as he was still delirious, he was given 15 grains of trional. Temperature 104.2°.

Saturday (10th). Temperature 103.4°, pulse 120, respirations 40. The respirations, though increased in rate, were quiet and easy, and there was no alteration in the condition of the lungs. He remained during the day quietly delirious, taking his nourishment well and causing no trouble or anxiety. Towards evening; however, the delirium increased, and by nine o'clock p.m. it had become noisy and of the "busy" type, the business of his "public house" giving him great concern. He became more and more restless and then violent, so that the two residents could not control him. His temperature had at 10.30 run up to 106.6°, and he had to be put into a strait-waistcoat, and was given a quarter of a grain of morphine; this had no effect, so $\frac{1}{100}$ grain of hyoscine was given. This quieted him, and the waistcoat was immediately taken off, having been on half-an-hour, and he was tepid-sponged as the

temperature was still 106.6° at 11. He was quite unconscious. The temperature kept still rising, so that by 12 o'clock midnight it was 107.6°. He was now (Sunday 11th) cold-sponged every half hour, being covered with a cold damp sheet in the intervals. The temperature soon fell to 104.4°, and remained at that during the early morning hours, and between 6 and 7 o'clock a.m. he again became conscious, but the temperature was again 104°, the pulse very rapid, uncountable and weak. He soon again became unconscious, and died at 12.20 p.m.

Post-mortem examination confirmed the conditions made out during life. The lower six inches of the ileum showed large and congested Peyer's patches and solitary follicles, those near the cæcum having commencing superficial ulceration.

The case is of special interest in several points :—

(1) The acute onset with continuous high temperature—103°-104°—from the evening of admission, which was apparently the sixth day of disease, till it terminated; with a fatal exacerbation on the tenth day to 107.6° F.

(2) The diagnosis of enteric positively supported by the atropine test without delay and with great ease.

(3) The appearance of "delirium ferox," which appears to be a very fatal symptom in this disease.

(4) The confirmation of the diagnosis by *post-mortem* examination, where the intestinal lesions were seen in an unusually early stage.

SOME MEDICAL PROBLEMS OF THE PRESENT AND THE FUTURE.

By WILLIAM M. CROFTON, M.D.;
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[Read in the Section of Medicine, February 23, 1917.]

It appears to me that it may be useful to review briefly the efficiency, or otherwise, of our present methods of prevention and treatment of certain diseases which are of great public interest, since they cause a high degree of mortality and inefficiency.

I take it that the larger question as to whether the abolition of disease would or would not be a desirable thing if we could accomplish it, is no concern of ours as practitioners of medicine; our duty is to keep the individual an efficient member of the community during the normal span of human life. That we have our work cut out if we are to accomplish this is evident, for if we consult the report of the Registrar-General for Ireland for 1914 we see that of the 71,000 odd deaths more than 47,000 occurred before the age of 70.

It is impossible, of course, to do away entirely with death before 70, but there can be no question that it would be a desirable thing if this pre-seventy death-rate could be reduced, provided always that the individuals who would have died are efficient, capable of supporting themselves,

and doing useful work for the community. The fact that the individual living in a society has a duty to that society outside his own personal interests is an idea that does not seem to strike the majority of people. I am proud to say that it is more intensely felt and acted on by the members of our own profession than by any other class.

Let us now examine the chief causes of death. Old age heads the list, next comes tuberculosis, next heart diseases, next bronchitis, next pneumonia, and then cancer, which in 1914 killed nearly as many people as pneumonia. Then there are such diseases as rheumatic conditions and others which, while they do not kill, at once produce prolonged disability and inefficiency, and diseases connected with reproduction which not only produce inefficiency in the generation that acquired them, but hand on this inefficiency to the next generation. I do not propose to discuss in this paper the question of infant mortality, which is largely due to maternal ignorance. I wish especially to emphasise and consider methods of prevention which are easy, and also what methods we have, or may in the future have, of cure, which is much more difficult.

There are some fundamental principles which we must be clear about before we proceed further. The great majority of diseases, at any rate in non-tropical countries, are due to parasitic unicellular fungi of the order schizomycetes, or, as we call them, microbes. Now, we cannot think that these microbes were created with the sole purpose of keeping down the population. The real duty of microbes is to break down after death the large organic nitrogen compounds into the simple inorganic nitrogen compounds, which can then be used as food by plants which build up again the large organic nitrogen compounds, which can be used as food for another lot of

animals. Unfortunately, certain microbes—for microbes have an extraordinary power of adapting themselves to their environment—have acquired the capacity of breaking down the *living* protoplasmic molecule, and this they accomplish by means of ferments, which, because they poison us, we call toxins. If the living molecules were helpless against these toxins then no life could exist, because we should all be split up into our elements, being killed by the pathogenic microbes, and the process of breakdown completed by the saprophytic microbes, the same microbe often accomplishing the whole process. But living protoplasm has the power of reacting and producing anti-bodies to the microbes' weapons of attack, and so acquiring complete or incomplete immunity. Always we have microbes in us capable of attacking our tissues if from any cause their capacity for making anti-bodies, or, as we call it, their immunity is lowered. We see, then, that health depends on the balance of power being kept up on the side of our protoplasm. If it is upset in favour of the microbes, we suffer from disease. Normally, we can without evidence of disease overcome and kill off the microbes which are continually invading us, but if a microbe has developed a specially powerful toxin it can break down our normal resistance—a breakdown which may be temporary or permanent. Or a microbe of normal virulence may invade us if our resistance is lowered by other causes, such as previous invasion by another microbe or chemical poisons, such as alcohol, &c. We have, then, always in us microbes ready and capable of splitting us up into our elements if our resistance is lowered. In our intestinal canal is always present the bacillus coli; this microbe and others are being continuously carried through the intestinal wall, and we know that the bacillus coli is capable of be-

coming pathogenic, for in the typhoid and para-typhoid fevers it is it that produces the deep ulceration and perforation, &c. In other diseases, such as appendicitis, it is always present as a secondary, if not a primary, invader. These intestinal microbes are, however, not entirely useless, for they help in the digestion of some foodstuffs which are otherwise indigestible, such as cellulose. When they are over-active they allow, by means of other microbes and by their own action, of the production of abnormal breakdown products of proteins, &c., such as aromatic amine compounds, which are poisonous when absorbed. Chronic infection and poisoning from the bowel is the common cause of arteriosclerosis and like diseases which produce premature senility. They are responsible, when not kept in their proper place, for a great deal of ill-health, so that one of the most important points to make sure of, if we want to keep efficient during the allotted span of useful work, is to take care that our resistance to these normal inhabitants of our intestines is adequate. Metchnikoff recommended for this purpose the establishment in our intestinal tract of microbes which would produce acid, such as the lactic acid forming microbes produced in quantities by culture in milk. Lane's plan is to cut out the large intestine! The first plan is a feeble one, the second absurd. Much better than either of them, when there are symptoms of bacterial overaction, is to increase the patient's resistance by artificially raising his immunity by means of vaccine made from these generally considered innocent inhabitants.

Let us turn now to tuberculosis. Five years ago, when giving this address, I stated on the authority of Prof. Pearson that while the death-rate from phthisis was falling, it was falling at a sensibly diminished rate as com-

pared with the general death-rate; indeed, that instead of falling it was showing in the case of females a slight rise. Since then this retardation has continued, so that what has been done, and is being done, for the prevention and cure of consumption is so far futile. My argument then was, and now is, that while for practical purposes every boy and every girl who is born into this world alive gets infected before reaching adult life, only a small percentage develop the disease because their immunising mechanism is normal, and, therefore, that if we could make sure that the immunity of those who would otherwise develop the disease was normal, then the disease would only occur sporadically. I said that there was only one way of insuring this, and that was by raising the resistance of the individual by preventive inoculation, and that this was particularly necessary in the case of the children of tuberculous parents and in families in which tuberculosis had occurred.

Again I say, with every sense of responsibility, that since the tubercle bacillus is so all-pervading, tuberculosis will not be stamped out without preventive inoculation. I wonder how long it will take for this measure to be adopted; probably not until the idea comes as a wonderful discovery from some foreign country. Then the authorities, and the *savants* of our profession, will be quite pleased to follow after the great foreign authority. And yet the measure is perfectly logical. The inoculating material is a dead toxin, not a live microbe. It is perfectly safe if carried out according to the directions I have laid down elsewhere, and it has the additional advantage of discovering incipient disease at a stage when it can be cured with ease and precision.

Next, with regard to treatment. Have any advances been made in the treatment of the disease? In my former

address I pointed out that tuberculosis of the lungs was, for all practical purposes, a mixed infection, the tuberculous infection being complicated by infection with catarrhal microbes, and I suggested as a treatment a combination of chemo- and immuno-therapy which was giving me promising results. I advised the administration of iodoform intravenously in solution in ether, a course of autogenous vaccine made from the patient's sputum being given at the same time, and this followed by a course of tuberculin. Since then I have been able to try this method at the National Hospital for Consumption, and I hope you will think the results during the short time the complete method was in use promising. Taking all the cases, the number of "apparent cures"—that is, those who had lost active signs in the lungs and those in whose sputum tubercle bacilli were not found in many examinations—was just trebled, and for the first time 5.6 per cent. of advanced cases were apparently cured. In 1912, before the treatment was employed, none were "apparently cured." Since the outbreak of war the treatment has had to be much modified, but in the early cases the apparent cures have gone up from 10.9 per cent. in 1912 to over 41 per cent. in 1915; in second stage cases they were 6.6 per cent. in 1912, 21.6 per cent. in 1914. The more advanced cases did not do so well during 1915, as it was impossible to give them the same careful individual attention as in the first eight months of 1914. By these methods I have been able apparently to cure several acute cases, who remain well after some years, and to arrest many advanced cases—cases with the whole of one lung and part of the other involved and having cavities. Of course, the more advanced the case the worse the prognosis, but I claim to have given a percentage of these advanced cases a chance of recovery and of a return to useful work.

Financed by the Medical Research Committee, Mr. Algar and myself are working to improve the chemotherapy of the disease, and in di-iodosalicylic acid we have found a drug which appears to give improved results. But I say nothing definite about it yet. I am sure that the solution of the tubercle bacillus in benzoyl-chloride is a better tubercular antigen than any of the other tuberculins I have used, and I am glad to say others who have used it are of the same opinion.

But as to the general aspect of the campaign against the disease, has the campaign been properly organised, or is the whole thing being done in a half-hearted and inefficient way? I am afraid that the latter is the case. Some years ago I submitted a complete scheme to the Local Government Board, which included county tuberculosis officers, county hospitals, with resident medical officers under the command of the county officer, and suggested the holding of tuberculosis dispensaries by dispensary doctors, who should have special post-graduate courses of training and special tuberculosis nurses to help them. The big cities were to have their own special tuberculosis hospitals near them, and convalescent houses in the country for the hardening process and completion of cure; while in the capital there should be a research institute where also the tuberculin would be made. Instead of such a complete scheme, which would give some chance of success, the money available has been used in building unsuitable sanatoria on unsuitable sites, where the treatment used has proved of little permanent use, as shown by the Registrar-General's figures and the statistical inquiries of Prof. Pearson. If we are to win the fight against the disease, we have got to be properly organised and efficient.

Let us now turn from this vexed and vexing question

to the next great cause of death—viz., heart disease. When the structure of the heart is changed by disease cure is impossible; all that can be done is to keep it going in its damaged state. But can we not do more than we are doing to prevent the heart becoming damaged, or to reduce this damage to a minimum? The great causes of damage to the heart are infections, the most common infections being various kinds of streptococci, which infect the heart when they are circulating in the blood in acute and chronic rheumatic conditions, scarlet fever, and so on. Of course, we cannot prevent the infection in the case of such a disease as scarlet fever, but by giving up the expectant treatment and treating the disease with sera, vaccines and chemotherapy we can do away with focal infections in the heart and kidneys. The same argument holds in rheumatic conditions. Salicylates no doubt are most valuable for the relief of pain, but if they are germicidal at all they are at best but feebly so. I have little doubt di-iodosalicylates are much more powerful, especially when given intravenously. Autogenous vaccines, too, should be used. In these ways the causative microbes will be entirely killed off, and relapse, which is so constant after this disease, prevented. But preceding rheumatic fever there are nearly always foci of infection, especially in the tonsils or gums. If these were treated and cleared up, acute and chronic rheumatism would not occur. Chronic rheumatism is nearly always complicated by intestinal infection, with consequent abnormal metabolism of foodstuffs. This intestinal infection must always be taken into consideration when treating the disease.

Our aim, then, is to prevent heart disease, since cure is impossible when the structure of the heart is damaged.

If we take the acute and chronic infections of the lungs

other than tuberculosis, together we find they beat even this disease as a cause of death. And this is not surprising, since the ordinary treatment for these diseases is purely symptomatic, and the symptomatic treatment, although quite recognised, is not always unharmful. For instance, are not ipecacuanha and antimony almost used as a routine treatment for bronchitis and broncho-pneumonia in children. They are commonly given in large doses to make the child vomit and so get rid of the mucus in his tubes. But what if the child does not vomit? These drugs in large quantities are severe heart depressants, and often lead to death from heart failure. Fewer children will die of acute bronchitis if a simple mixture of squills and spiritus æther nitrosi is used. As a rule, children recover permanently from these acute conditions. If they do not, vaccines made from the flora of their sputum will rapidly cure them permanently. The same holds good for acute bronchitis and broncho-pneumonia in adults. In them, chronic bronchitis as a sequel to the acute is the rule rather than the exception. If these cases were treated with autogenous vaccines from the first they would not become chronic. A person with an uncured catarrh of his nose, bronchi, tonsils or gums is in danger of his life. He is constantly infecting his intestinal tract, and these microbes are constantly passing into his system, so that any organ of his body may become infected, and if from any cause his resistance is suddenly lowered he will have an acute infection which may kill him.

With our present therapeutic resources no catarrh need ever become chronic.

The microbes capable of producing these acute catarrhs are so numerous that it is not feasible to use prophylactic vaccines unless a chronic catarrh is present.

I do not suppose there is any disease more dreaded than pneumococcal pneumonia, nor is this surprising, for we have at present in our armamentarium no certain method of killing the pneumococcus intravitally. True, there are good sera to be had against some strains of the pneumococcus, and these are very effective if the one used happens to hit off the particular strain; a serum should, therefore, always be used if available. Pneumococci vary so much in the quality of their toxins that I do not think it possible that a universally effective serum is likely ever to be obtained. I think rather that an intravital germicide will be found which will produce the desired result. I would much like to try di-iodosalicylic acid, which, as I have already mentioned, gives good results with other members of the streptococcal group. In pneumonia, an autogenous vaccine should be used as soon as ever it is available. It is very disappointing that ethylhydrocuprein, which gave such promising results in the animal experiment, has not done so well in the human disease.

I now approach the realms of high romance—the problem of the prevention and treatment of cancer, which seems to be killing an increasing number of men and women every year. Have we any glimmerings of the cause? None of the researches on the transmission of sarcomata or carcinomata in animals seem to have thrown any light on the subject, except, to my mind, in this way: that if the same tumour is inoculated into a number of similar animals at the same time, it will only develop in certain individuals; the rest kill it off. Why does it develop in one individual and not in another? Because in one the environment is suitable, in the other it is not. The same argument applies to spontaneous cancer. There is always some source of irritation—microbial, mechanical,

chemical, or other; at the site of this irritation the tumour develops. But all people who have these sources of irritation do not develop cancer. Why? Because the environment is not suitable. Most probably we shall be able to find out before long what the change in the blood and tissue juices that constitutes a suitable environment is. We shall then be able to test whether a patient would be likely to develop a cancer if a source of irritation arose, correct the metabolic change, and so make it impossible for a cancer to develop.

It is possible, at any rate, that this suitable environment is brought about by abnormal metabolism of food stuffs, produced by microbes in the intestinal tract, perhaps a chronic acidosis, or something of the kind.

It is quite recognised that cancers commonly develop at the site of chronic microbial activity—for instance, at the site of a chronic ulcer in the stomach, or in a cervix chronically inflamed by microbes. Such infections need seldom continue, for they can usually be cured by vaccines with precision. You will be quite clear that I do not consider that cancer is caused by a special microbe; I am only suggesting that microbes may produce one of the kinds of irritation which sets a cancer going, and that microbes may be a factor in producing a suitable environment. Beard's hypothesis that cancer represents an asexual generation to my mind has much to recommend it.

At any rate, what I have said about the ætiology of cancer gives a reasonable hypothesis on which to work.

Just a word now about venereal disease. It seems to me that a great deal of mischief arises from the mystery that surrounds the facts of reproduction. Parents are afraid to tell their children the simple and beautiful facts of paternity and maternity. Children are not taught that

one of their most sacred duties is to hand on their germplasm undamaged to the next generation, nor are they taught that illicit sexual indulgence will most likely lead to disaster to themselves and to their children.

I have said enough, I think, to indicate that there are many fields to conquer, that we have the means of conquering some of them, while for others the means have yet to be found.

Let us now, before I close, consider what position we are in in Ireland to investigate disease and work out cures for it. There is only one institution I know in Ireland where the organisation is such that it gives a chance of really good work being done—and even it might be better—where the head of the hospital is a whole-time man of great merit, who can decide on a settled policy and have it carried out by efficient whole-time assistants, and that is the Rotunda Hospital; and the result is that this institution is world-famous for its good work. Until we apply such a system to our general hospitals and elaborate it, or institute a special research hospital with a whole-time director and staff, I see little hope of doing really effective work in our fight against disease; for the investigation of a case is becoming a more and more elaborate affair, involving investigation not only of symptoms and physical signs, but also of microbial inhabitants, and chemical examinations of blood and excreta; and very likely in the future we shall be investigating the physical phenomena of the colloids of which the patient's body is composed.

Dr. T. P. C. Kirkpatrick considered the reported increase in the number of "cured" cases at Newcastle extremely satisfactory. He would like to know whether Dr. Crofton considered the treatment adopted to be entirely responsible

for this, and also whether the cases were regarded as permanent, as almost any line of treatment seemed successful at first. He thoroughly approved Dr. Crofton's remarks as to the lack of organised and systematic medical research in Dublin at present, and mentioned that some steps had already been taken to improve matters in this respect.

Dr. T. G. Moorhead agreed in theory with much of the paper, but pointed out many practical difficulties. Prophylactic inoculation against tuberculosis must, in the present state of our knowledge, compare very unfavourably with well-established methods of protection—such as vaccination against smallpox. How long would immunity take to develop or how long would it persist? In speaking of the "cure" of pulmonary tuberculosis some standard was required. He had recently watched four cases get well on various treatments—two on none beyond general hygiene. He thought the efficiency of vaccines was often exaggerated.

Dr. G. E. Nesbitt thought the paper unduly optimistic. The claim that the causative organisms could be destroyed in all cases by the methods advocated was not borne out by universal experience, and the foundation of a widespread system of protection on this theory was to a large extent begging the question.

Sir John Moore recollected how Stokes often related that one of his most successful cases of recovery from tuberculosis had been a patient whom he had sent away as hopeless, to live as he liked. It was extremely difficult to draw deductions as to the efficacy of any treatment of tuberculosis merely from statistics of apparent cures.

Dr. Crofton, in reply, said the criterions of "cure" at Newcastle were complete freedom from cough and absence of tubercle bacilli from the sputum, preferably after several examinations.

CLINICAL NOTES OF A CASE OF INTRATHORACIC TUMOUR.

By GEORGE E. NESBITT, M.D. (Dubl. Univ.), F.R.C.P.I.;
Assistant Physician, Richmond, Whitworth and Hardwicke Hospitals.

[Read in the Section of Medicine, February 23, 1917.]

P. B., aged sixty-five, was admitted to the Whitworth Hospital on the 25th of November, 1916, complaining of bad cough and some difficulty in breathing.

The history of his illness was obscure. He said, though he had always had "a bit of a cough," he had enjoyed good health, except for an attack of facial paralysis, about six years ago, which had left some traces in the shape of slight contracture and deformity of the right side of his face.

On cross-examination, however, he admitted that his cough had got distinctly worse about three months previously; that recently it had been very severe and constant; and that he brought up a good deal of spit—on one occasion blood-tinged.

Although his temperature was 101° when his daughter insisted on his seeking advice, he complained singularly little, and he was with difficulty persuaded to come into hospital.

Next day I had an opportunity of examining him thoroughly.

He was a thin, scraggy, untidy-looking individual, and was decidedly pale in colour. He was running an evening

temperature. The various systems, with the exception of the chest, were not markedly abnormal, but here some anomalous symptoms were at once detected.

The chest was fairly well shaped, but exhibited a very small range of expansion (about three-quarters of an inch), the diminution being perhaps more marked on the left side. Vocal fremitus was considerably diminished over the left lung. On percussion both lungs were resonant, but there seemed a slight difference of pitch over the upper lobes—the left side being slightly higher. Resonance at the bases was hard to demonstrate, and seemed defective on the left side. On auscultation there was entire absence of breath sounds over the left lung—those over the right lung were loud and harsh, but not accompanied by any adventitious sounds.

The apex beat was in the nipple line, cardiac dulness being slightly increased to the left, and the heart sounds were rather feeble. The pulse was 80, and showed some thickening of the arterial wall.

The remarkable phenomenon at this stage was the combination of resonance with entire absence of breath sounds. On listening to the chest the first impression gained was that we were dealing with some condition causing collapse of the lung—*e.g.*, effusion or pneumothorax. These, however, could soon be excluded. My first view was that something had completely blocked the left bronchus. There was, however, no history of gradual stridor, and I could not imagine that a lung with the bronchus occluded would long remain resonant. I therefore fell back on the theory that the left diaphragm was not acting. This view was supported by the almost complete absence of movement in the left side of the chest and abdomen. Assuming this to be the explanation of the signs

observed, the diagnosis of something paralysing the phrenic nerve was a logical sequence, and from this to a diagnosis of malignant growth in the left mediastinum was an easy transition.

At this point an *x*-ray examination was kindly made by Dr. Hardman. He reported "a large sharply-defined shadow spreading out from left root and extending up into apex of lung—suggestive of new growth."

On a subsequent careful examination no movement of the left diaphragm was observed and practically no increased illumination of the left lung on inspiration.

During the next ten days the physical signs changed rather quickly. Dulness appeared in front opposite the root of the lung, and spread rapidly over the chest. The signs were now those of a pleural effusion. An exploring needle, however, revealed pus. Sir Thomas Myles then drained the chest, and, as he had confirmed the presence of pus by needle, immediately before operation, was surprised to withdraw a large quantity of serous fluid.

On further gentle exploration a small localised collection of pus was evacuated—whether an empyema or a lung abscess could not be definitely ascertained. During the following week the patient ran a satisfactory course, but then, without warning, he collapsed and died.

Autopsy revealed a neoplasm (lympho-sarcoma, specimen shown) involving the left bronchus (which it had almost completely occluded) and branching radially outwards through the lung. The pus was a small empyema. No secondary growths were found.

CLINICAL NOTES OF A CASE OF SEPTIC PNEUMONIA IN THE PUERPERIUM.

By SPENCER SHEILL, F.R.C.P.I.
Examiner in Midwifery and Gynæcology, Conjoint Board R.C.P.S.I.;
Late Assistant Master, Coombe Maternity Hospital, &c.

[Read in the Section of Obstetrics, March 2, 1917.]

Not many decades have slipped by since obstetricians were accustomed to look upon serious complications of the lying-in bed, and even its fatalities, as commonplace. This sad state of affairs and the attitude of the profession to it were mainly due to ignorance of the cause of sepsis, and it occurred at a time when specialists in the art of obstetric surgery were unknown.

It cannot be much over 30 years ago since an American physician of repute said that "puerperal fever was due to the will of God," and was beyond the power of man to prevent. Time may never disprove the former part of his statement, but it certainly has, in the large majority of cases, proved the fallacy of the latter.

Nowadays, except in the most remote parts of our country, we have come to look aghast upon complications of parturition and the puerperium, and upon fatalities in child-birth as nothing short of a tragedy. Who will dare to say that the loss of a life whilst giving a new life—in itself the gift of a sacred union—is anything less than a national calamity, and at the same time the most appalling domestic tragedy we can contemplate?

Thanks to the work of Pasteur and Lister, puerperal

uterine sepsis is comparatively seldom met with at the present time; yet even much rarer is the type of sepsis of which I am bringing forward the notes of a case. I have never seen an instance of it before, neither have I met with anyone who has.

I do not care to dwell much upon the event, so painful to me are the details. At the same time, I consider it my duty to the profession and to future patients to expose it to the full light of day, in the hope that by so doing some new light may be cast upon it, to show me if I erred, and if so wherein, and thus others of us might benefit by this most tragic case, and good arise out of evil.

The patient, a primipara, was an extremely thin and delicate-looking young woman of about 28 years of age; at the same time she was vigorous and wiry in nature. Her family history was none too good, her sister having had eclampsia and puerperal mania of a severe type in her first confinement, from which she eventually made a complete recovery.

My patient and some of her relatives anticipated some such trouble in her case too, but for this gloomy view there was no foundation in fact other than her sister's history, her own temperament, and her past tendency to suffer from sore fingers, furuncles, and such like evidences of lowered resistance to infection. The labour at term was comparatively uneventful, she delivered herself normally —if a little tediously—of a healthy child, the usual amount of chloroform being given towards the termination. There was a perineal tear requiring two sutures of silkworm-gut, and these were removed on the seventh day, good union resulting.

The patient got up on the ninth day feeling very well except for a tender spot on the right breast; this spot

looked more like a cutaneous pimple than a threatening mammary abscess, so small was it.

The same evening her temperature rose to 102° F., the first rise of temperature during her puerperium.

I kept her in bed and ordered antiseptic wet dressings, while the inflamed spot increased in size towards the nipple until it approached the size of a 5s. piece. On the eleventh day, whilst applying suction to the whole breast with a Bier's apparatus, one or two drops of what undoubtedly looked like pus, were drawn from a milk-duct. The next day local œdema and slight fluctuation were apparent, so I opened the breast under nitrous oxide anæsthesia, and evacuated a small amount of pus and altered milk; the incision was explored digitally to make certain that no pus pockets remained unbroken. I might say that at no time was there any sign of crack or chap on or near the nipple. Contrary to the usual experience, her temperature did not fall to normal after the operation, but instead rose in two days to 104.4° F., although the breast was doing very well, *except for one detail*, to which I would particularly draw attention, *the œdema did not subside*.

Suspicious of her lungs, I made an examination of them, and found well-marked patchy sings of pneumonia. At a consultation a few hours later my suspicion of "septic pneumonia" was confirmed, and a gloomy prognosis agreed upon.

Six days after the opening of the abscess, and the eighteenth day of her puerperium, she died of acute septic double pneumonia. The last four days of her life were marked by acute violent mania; identical with the type her sister had suffered from, and by no means like the delirium which commonly accompanies fever. She fought

with the nurses, and threw things about; she talked with the "cute" sanity and clever wit of a true maniac, after a time falling back upon the pillow exhausted with her exertions and panting for breath.

I think some points of interest are raised by this case.

1. Its rarity.
2. Its fulminating type.
3. Its being uncomplicated by sepsis elsewhere than the breast.
4. The failure of the breast operation to make the local œdema subside.
5. The mania. And last, but by no means least, the want of explanation as to how and why septic matter found its way into the general or pulmonary circulation.
6. To this last conundrum I intend, in my reply, to throw out, for what it is worth, a suggested answer. Possibly some of you will anticipate me in this; if so, I shall value your opinions all the more, knowing that you have independently suggested a similar cause for the disaster, and, therefore, given to the profession a means for avoiding this subtle pitfall.

The President of the Academy said.—Our interest in Dr. Sheill's case is not a little deepened by its tragic ending. To me it appears that the breast condition was a local evidence of a general septic condition, which developed with accelerated speed after the abscess was treated. I recall the fact that Rokitansky and McClintock recorded cases of puerperal sepsis, attended with suppuration in the breast.

Dr. Bethel Solomons considered that the case was most likely to be one of embolic pneumonia, secondary to the mastitis, while the possibility that the breast symptoms were evidences of general sepsis was also a very feasible hypothesis. He thought it very unfortunate that Dr. Sheill had not attempted to get a culture from the mammary pus or

from the uterus, as an autogenous vaccine might have helped in treatment.

Dr. Drury showed some interesting charts of patients who had developed pneumonia during pregnancy. The prognosis of pneumonia in pregnancy is favourable for the mother and bad for the child, as premature labour often occurs. Labour has a beneficial effect on the disease. He quoted the pneumonia statistics of West and Thomas, and said the disease was rare among puerperal women.

Dr. Crofton thought Dr. Sheill's case was interesting from three points of view: first, the source of the infection; secondly, the possibility of uterine infection in the puerperium being caused in some cases by a chronic septicæmia, without symptoms; and, lastly, the connection of acute infection with pneumonia.

Dr. Sheill, in reply, stated his belief that septic emboli from the breast caused the trouble, and that these were admitted to the circulation by the use of Bier's suction apparatus, which created such powerful suction as to rupture some small veins, and that when the pressure was released septic matter was drawn into these veins. He, therefore, considers the use of the Bier apparatus to be fraught with considerable danger in cases where the abscess does not freely communicate with the surface.

SOME OBSERVATIONS ON THE OPERATIVE TREATMENT OF FIXED BACKWARD DISPLACEMENT OF THE UTERUS.

By ALFRED SMITH, F.R.C.S.I.;
Professor of Midwifery, N.U.I.;
Gynæcologist, St. Vincent's Hospital, Dublin.

[Read in the Section of Obstetrics, March 2, 1917.]

The operative treatment for fixed backward displacement of the uterus is one of the most gratifying advances in gynetic surgery. If you, about a decade ago, asked any gynæcologist what was their treatment for a fixed backward displacement uterus, he would advise you to use either ichthyol and glycerine plugs, hot douches, mud baths, or pelvic massage, &c. Now a laparotomy is performed, adhesions are broken down, and the uterus is suspended or fixed at the choice of the operator.

Has finality been reached or has the last word been said when you have suspended or fixed the uterus? The class of fixed backward displacements that I had to deal with was that caused by pelvic peritonitis, the uterus being held down by fibrous adhesions, complicated in many cases with prolapsed and fixed appendages. My routine treatment was to separate the uterus from the adhesions, straighten out the tubes, free the ovaries, resect when necessary, and draw loops of the round ligaments through the recti muscles, stitch them there after the manner

recommended by Dr. Gilliam. The "end results" were not satisfactory. Many patients afterwards complained of dragging pains; referred to the suspension points in the recti muscles. I thought that perhaps my technique was faulty.

I soon found that all cases could not be treated alike, but that each should be treated on its merits. The behaviour of the uterus was different after the separation of the adhesions. In some cases the uterus came easily up to the abdominal wall, showing a certain amount of relaxation of its supports. In others it did not come up so easily, but some considerable degree of force was necessary to draw it up into position. Thus, I was able to divide my cases into two groups: (*a*) The uterus with relaxed supports. (*b*) The uterus with unrelaxed supports.

Group (*b*) should be treated quite differently from group (*a*). The uterus with relaxed supports—group (*a*)—must be suspended or fixed. Suspended preferably during the child-bearing age; fixed when the climacteric was passed.

On freeing uteri with unrelaxed supports three types were met with: (1) Where the uterus righted itself automatically; (2) where manual replacement was necessary; and (3) where, owing to a thickened and shortened utero-sacral ligament, the uterus could not be brought into the normal position of anteflexion. Types (1) and (2) have little tendency to fall back; suspension or fixation is therefore unnecessary.

As there is always a certain amount of interference with the blood circulation in fixed displacements of the uterus, I find it advisable to place a gauze tampon in the vagina, so as to tilt the cervix backwards. This enables the blood circulation of the uterus to become normal. Tampons

should be removed at the end of twenty-four hours, the vagina irrigated, and a fresh tampon introduced.

The treatment of type (3), fixed displacements, with thickened and shortened utero-sacral ligaments baffles me. I merely free the uterus from its entanglements, straighten out tubes, release the ovaries where necessary, and leave the rest to nature. It would be hopeless to attempt suspension or fixation.

The unsatisfactory "end results" previously mentioned of the dragging pains referred to the suspension points in the recti muscles was now explained. I had been suspending uteri with unrelaxed supports when I should have left them alone.

I bring these observations before you in the hope of interesting you in determining the limitations of suspension or fixation, and of putting the operation treatment of fixed displacement on a scientific footing. Few of the modern works deal with the question. Dr. Jellett, in his 1916 "Practice of Gynæcology," recommends the breaking down of adhesions with a view of correcting the malpositions which they cause, and in the ventral suspension or fixation of the uterus.

My classifications and groupings are purely provisional. Judging from the literature at my disposal, this subject has not been fully dealt with. Possibly, members may give me references. At any rate, we have ample material in our clinics at the Rotunda, Coombe, and Holles Street Hospitals to once and for all determine and give a lead as to the correct principle that should govern gynæcologists in their treatment of fixed backward displacements.

Dr. Hastings Tweedy said that he too had found great difficulty in dealing with the class of cases described by Professor Smith. They usually showed a shallow pouch of

Douglas and very hard and unyielding sacro-uterine ligaments. The condition arose from a previous peri-metritis, and affected the structures included between the sacro-uterine folds.

He was in the habit of cutting these folds down to their basement connective tissue. By this he frees the uterus without injuring the uterine support, for the uterus owes its entire stability to the utero-peritoneal connective tissue, which lies at the bases of the so-called ligaments. The fibrous bands are intimately connected with the uterine muscles, and form tendons to them.

The President of the Academy said.—The subject treated in Dr. Smith's paper owes its importance to the frequency of its occurrence amongst the serious sequelæ of parturition, the impairment of general health which often attends it, and the partial success which is all that sometimes attends our treatment of it. I think Dr. Smith's classification of such cases is a useful and practical one, calculated to aid us in selecting the treatment best suited to each case.

Dr. Bethel Solomons thought that in all cases of fixed backward displacement which were treated by operation the uterus should be suspended in the manner suitable to the case. He considered that the mere loosing of adhesions, as suggested by Dr. Smith, was not enough to bring about a permanent cure. Where there was a tendency for utero-sacral ligaments to exert tension on the uterus which was suspended, tamponnade, by means of medicated vaginal plugs, would cure this inflammatory condition. He deemed it advisable to curette the uterus in addition to correcting the malposition.

The President said the classification which had been made was very necessary in the treatment of cases. He had adopted the method of suspension of the uterus to the abdominal wall for some time, but came to the conclusion that it was unsatisfactory to bring the fundus forward to a fixed degree in all cases, and now performed a modified Gilliam operation, as thus the fundus could be brought forward sufficiently to prevent retrogression without putting undue tension on those uteri which would not come into complete

anteversion. He also considered it most essential when the uterus was brought forward to see that the appendages did not fall back again, as they often have loose ligaments. The Gilliam operation has the advantage that it takes up some of this slack of the broad ligaments, but if not sufficient the ovary should be fixed to the top of the broad ligament.

A NOTE ON THE LIMITATIONS OF VACCINE TREATMENT.

By ROBERT J. ROWLETTE, M.D. (Dubl.); F.R.C.P.I.;
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[Read in the Section of Medicine, March 30. 1917.]

It is my aim in this note to sketch very briefly the place that vaccines should take in therapeutics, and to consider what may fairly be expected of vaccine treatment, and what are its limits. I am aware that the observations I have to make are obvious and even commonplace, but I have often heard them overlooked, both in public discussion and in private conversation.

It is notorious that no judgment in medical practice is more difficult than the estimation of the value of a particular method of treatment. A new method is introduced, it wins a certain amount of favour, it may become the fashion, and unless it is accompanied by some obtrusive disadvantages or dangers, many of us are convinced that the treatment is, if not "the last word," at least "a distinct advance," and we go on with it until a new fashion takes the place of the old. Our reasoning on these matters is never logical, and rarely individual. We are governed by laws—if they be laws—of herd psychology. Our conclusions are instinctive, not conscious.

This is, in part, due to the difficulty of forming a judgment of cause and effect in a highly complex set of phenomena. It is difficult or impossible to arrange a

therapeutic experiment in such a way that the conclusion is irresistible. The problem is not so complicated in surgical as it is in medical treatment, since the conditions more closely approach those of scientific experiment. Nevertheless, even surgeons are not free from the influence of fashion—that is to say, from the sway of inaccurate judgments. Gastro-enterostomy, appendicectomy, colectomy, Jackson's membrane, have all had their vogue, and the muciform sac is not the exclusive specialty of an imaginary surgeon.

In every branch of Medicine we meet this difficulty of estimating therapeutic results, but in the case of vaccines the difficulty has been increased by certain more or less accidental circumstances. Vaccine treatment came to us as a gift from the laboratory. It came, therefore, with a certain scientific pedigree which had a claim on our consideration, but it was presented by men trained in laboratory methods, without clinical experience, and without the knowledge necessary for the comparison of therapeutic results. On their side, clinical workers, being unfamiliar with the technique of the preparation of vaccines, have been content to leave their administration to those who were. It has come about, therefore, that for years and, indeed, to a certain extent to the present day, vaccine treatment has remained in the hands of men of laboratory training. It is, however, clinical experience, and that only, that can seal its value.

The enthusiasm of the introducers of vaccine treatment —an enthusiasm often untempered by clinical experience—has put forward somewhat extravagant claims. These claims, misunderstood and misrepresented, led the optimistic to expect miracles. But miracles do not happen, and those who were disappointed on the one

hand, and on the other those who had their prophesyings of evil accomplished, agreed to declare that they had been hoaxed. The number of successes, however, was sufficient to keep up some belief in the miracles, and between the faithful and the sceptic there seemed to be no common ground. Sharp trading firms saw their opportunity, and by making impossible claims for inferior products caused both disappointment and disgust. In short, an abnormal state of mind was produced which has prevented the profession exercising a normal judgment.

We have, however, passed through both elation and depression, and we are, I hope, now in a position to judge coolly of the rightful place of vaccines in treatment.

First, as regards their place in Preventive Medicine. The experience of the present war puts beyond all question the value of preventive inoculation against the enteric diseases. Our troops have been inoculated against typhoid, but not at first against para-typhoid. We know how rare has been typhoid, although para-typhoid, hitherto regarded as a rare disease, has been, in some areas, by no means uncommon. Yet medical officers from most of the fronts have made us familiar with the grossly insanitary conditions under which the men have to live, and of the gross contamination to which much of the drinking water is subject. If our troops had to rely merely on sanitary precautions in the ordinary sense, and were not protected by inoculation, there does not seem to be any reason why they should not suffer from disease as troops suffered in South Africa, and in most other campaigns in history.

In our ordinary home-medicine we have also experience of the efficacy of preventive vaccination, particularly in the results of inoculation against recurrent catarrhs of

the respiratory organs. The problem is more complicated than in the case of typhoid, in that we are dealing with infections by organisms with which we are not so familiar, and, moreover, we are dealing not with a single organism, but with several, out of which it is a matter of difficulty to select the main infective agent. We have not, therefore, as good a record of success as in inoculation against enteric, but nevertheless successes are sufficiently numerous to justify us in the hope, that with a more intimate knowledge of the bacteriology of the respiratory tract, and more particularly with the development of knowledge of a correlation between the different forms of respiratory catarrh and different species of bacteria, we may soon be in a position to judge what class of cases may be satisfactorily treated, and what class we cannot expect to influence. It must not be forgotten that the attacks of some diseases, of which the best example is, perhaps, influenza, do not appear ordinarily to confer immunity, or at any rate an immunity which persists for any length of time. In the case of such diseases we are not likely to produce any persistent effect by preventive inoculation.

I have dealt with the question of preventive inoculation first for two reasons. In the first place, the claims made for vaccines are admitted. But in the second, the consideration of the prevention of disease by immunisation helps us to understand what we have a right to expect from vaccines in curative treatment. Active immunisation is the training and marshalling of the protective forces of the body against a particular kind of attack. That is to say, it is an attempt to stimulate a natural process—the process by which the body naturally defends itself against bacterial invasion.

A vaccine is not a new ally in the fight between the parasite and the host, assisting the latter to destroy the former. It is only a stimulus to the body to exert to the full its natural powers of resistance and protection. Of itself it can do nothing. Although, then, one rightly speaks of vaccine treatment as specific, it is specific in quite a different way from the so-called specific action of drugs. We suppose quinine to be specific for malaria, and arsenic or mercury for syphilis, because we believe the drug in each case to exert a special destructive action on the infecting parasite. But vaccines are only active against bacteria mediately in that they stimulate the production of bodies which possess special destructive powers toward particular organisms. Vaccines are, therefore, in a sense specific only at second hand.

As the action of vaccines is merely stimulative of certain natural powers of the body, it is clear that if the body is already exhausted, if it is no longer able to produce the substances, the vaccine fails. Moreover, in cases where it does not fail, its success is proportional to the power of the tissues to respond to its call. To expect uniform success from vaccines is, therefore, as unreasonable as to suppose that the body can never fail in its defence against attack.

It follows that the cases in which we may expect the best results of vaccine treatment are those in which (1) the body has not yet lost its natural power of resisting infection, and (2) in which there has not been great tissue-change. If the body has no power of resisting infection, the vaccine is quite inert. It is as useless as a trumpet-call in a deserted barrack-square. It is not possible to tell whether such cases really occur, nor how to recognise them if they do. We must, however, be prepared to meet

them, and not to be disappointed if the inevitable happens.

The second qualification is of more practical interest. When grave tissue-changes have taken place, we cannot hope by any method of treatment either to restore lost tissue or to cause the absorption of masses of new tissue of a degenerate order. Vaccines can only produce a resistance to an infective process—they have no effect in dealing with the results of that process after the process has ceased. I do not suggest that there are not other agencies in the body at work to restore damaged tissue to the normal, but these agencies are neither helped nor hindered by vaccines. Let me take one example :—

A Fallopian tube becomes infected by, let us say, the gonococcus, and suppuration follows. While the infection lasts, its progress may be hampered or prevented by vaccine treatment. But once the gonococcus loses its vitality, vaccines give no help to the tissues in their struggle to return to the normal. Neither will vaccines cure a stricture of the urethra consequent on gonorrhœa. This is so obvious as to appear commonplace. But let us apply the same reasoning to the effects of rheumatic infection, whether on the joints or on the valves of the heart. We may reasonably hope to hamper the progress of rheumatic injury or to arrest it altogether. But if gross tissue-changes have occurred, they cannot be repaired by vaccines. It would be as reasonable to suppose that if the invaders were to-morrow driven from the fields of Flanders, those fields would be restored forthwith to the condition in which they were three years ago. The battle may be over and the invaders destroyed, but many of the traces of battle are ineradicable, while others will persist for years. Louvain and Ypres will never

be again what they were three years ago. Neither will the heart valves that have been eroded and fibrosed in the battle between the infecting cocci and the protective agencies of the body be the same again.

The question is not infrequently asked—In what class of diseases should vaccines be employed? The answer, bearing in mind the limitations I have already suggested, is—every infective disease due to bacterial origin in which the bacterial cause can be discovered. The acuteness or chronicity of the condition is beside the point, though it may have much to do with the method of administration. A similar remark applies to the question whether the disease is general or local. In every case where a fight is going on between the body and an invading bacterium there is reasonable hope that the defensive forces will be strengthened by the administration of a vaccine. But, as vaccines are specific, a correct bacteriological diagnosis is necessary. In this, vaccines are at a distinct disadvantage if contrasted with other methods of treatment. By rest, suitable food, good nursing, and relief of unpleasant symptoms, we cure, or rather assist Nature to cure, many of our patients. If before treating them, an accurate diagnosis were necessary, how many successes should we have? But if vaccines are to have any effect at all, their use must be preceded by correct diagnosis. We must be sure that we are treating with the infecting organism. Of course, if the infection be mixed, as is so often the case, we must often be content to treat with a group of organisms—*i.e.*, with a mixed vaccine, being assured that at least one element in our vaccine is specific for the infection, and that the others, if not helpful, are at least harmless.

No greater misconception can exist than the notion

that vaccine treatment is incompatible with the use of other methods of treatment. On the contrary, one rarely gets the best results from vaccine treatment unless one makes full use of whatever concomitant measures his own experience or the accumulated experience of others may suggest. Few drugs are in any way inimical to the action of vaccines, and those that are are depressant to the natural resistive powers of the body, and are, therefore, contraindicated in the class of diseases in which vaccines are employed. Of these the only drug in common use is alcohol, the employment of which in septic diseases should by now be obsolete except under rare conditions. Vaccine treatment, therefore, goes side by side with treatment by drugs.

In surgical conditions, operative measures or anything else that surgical knowledge suggests should, of course, be practised when required. The only precaution to be observed is to withhold inoculations in immediate proximity to any manipulative proceedings which may give rise to auto-inoculation. Otherwise, vaccines, of course, act in their normal manner of increasing the protective power of the body.

I said at the beginning that much misjudgment of vaccine treatment by the profession has been due to the treatment being so largely committed to men unaccustomed to clinical work. It cannot, however, remain in the hands of a special class. As an important branch of therapeutics it must become familiar to every practitioner. No student can be considered to have gained a sufficient knowledge of therapeutics who has not become familiar with both the doctrines of immunity and the practice of immunisation. In this way vaccine treatment will come to take its proper place—not as something peculiar and

special, but as an essential part of the treatment of infective disorders of bacterial origin.

Dr. A. R. Parsons said that all were agreed as to the utility of vaccines in many cases, both prophylactic and curative. He had, however, been disappointed in many conditions—*e.g.*, in bronchiectasis, in which careful vaccine treatment had been a complete failure; and in cases of ulcerative endocarditis, where the organism had been recovered from the blood. It was hard to understand how good results could be expected where numerous organisms are actually present in the blood. He commented on the diminution in the use of alcohol in septic cases at the Rotunda Hospital, where it was formerly used freely.

Sir John Moore said the contrast drawn by Dr. Rowlette between the limitations of vaccine treatment and the effect of medicinal "specifics" is not of universal application. For example, while we recognise in the salicylates what may be considered a specific against the toxæmia of acute rheumatism, we know that they are practically useless when that toxæmia has produced organic changes—for instance, in the heart.

Dr. G. E. Nesbitt commented on the difficulty of recognising the causal organism in complex cases.

Dr. Rowlette, in reply, said that he thought in bronchiectasis the organisms present were saprophytic rather than parasitic. In general infection, while a theoretical explanation of the effect of vaccines was difficult to give, clinical experience had established their efficacy.

HIGH EXPLOSIVES, T. N. T : ITS TOXIC ACTION IN MUNITION WORKERS. DETECTION OF T. N. T. IN URINE. WITH EXPERIMENTS.

By WALTER G. SMITH, M.D.;

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Ex-President, Royal Academy of Medicine in Ireland;
Physician to Sir P. Dun's Hospital.

[Read in the Section of Medicine, April 13, 1917.]

The Twentieth Century, even in its youth, is a witness to rapid and extraordinary advances made in all branches of Chemical Science. A new epoch has unfolded itself. What a change when we now know that the atom, the foundation stone of chemistry, is not an immutable indivisible entity, but is a complex microcosm liable to sudden and forceful disintegrations, and forming a storehouse of vast potential energy.

Surely we may hope that Chemistry will soon come into its own, secure the ungrudging support of Governments, and infiltrate the general public with a sense of its fundamental importance.

This colossal and lurid World War has been described—not untruthfully—as largely a war of applied chemistry.

In this sphere of activity chemistry has been enlisted both lawfully and unlawfully. Lawfully, for the production of munitions of war and for the protection of human life in coping with the deadly chemicals employed by our adversaries. Unlawfully, and barbarously, for the maim-

ing and torturing of humanity by death-dealing poison gases, by the poisoning of wells, and by other measures of ferocity in defiance of all hitherto recognised canons of international practice.

From sheer necessity—greatly against the grain—the British and their Allies have been compelled to adopt certain measures of offence and of reprisal which are foreign to their nature.

As examples of gases used as asphyxiating agents may be mentioned chlorine, bromine vapour, phosgene gas ($COCl_2$), and sulphur dioxide (SO_2).

In addition to these familiar substances there are now employed a number of complex organic compounds which give rise to offensive and toxic gases, and combine the properties of being asphyxiating and lacrymatory. It is unnecessary to give their names or formulæ.

One thought that carries some comfort with it is that while war makes men devils it also creates heroes and saints.

Furthermore, the war has led us to recognise some new and serious occupational diseases arising from the novel chemicals introduced. And once again malingering has been favoured by a curious property of one at least of the modern explosives.

In this latter connection I refer to picric acid. Soldiers have learned to simulate jaundice by swallowing picric acid, which turns the urine orange-yellow, independent of bile pigment, and tinges the conjunctiva yellow, properties not possessed by any other drug known to me.

Some time ago I made a few experiments on myself. I took, within ten hours, three powders of two grains of picric acid, in cachets, without any harm ensuing. Yet it is stated (Sollmann, *Text-book of Pharmacology*, 2nd edit.,

1906) to be very poisonous, causing destruction of red corpuscles, nephritis, convulsions, and death through paralysis of respiration. 0.06–0.60 grm. is said to be fatal to rabbits and dogs.

Ammonium picrate has been recommended in malarial fevers, in doses of $\frac{1}{8}$ to $\frac{1}{2}$ gr.

The picric urine assumed an orange yellow colour, and remained sweet for a number of days. It was free from albumen and bile pigment.

Mr. E. Werner recognised that my conjunctivæ were tinged yellow. This soon passed off, and I was none the worse for the venture.

To detect picric acid in urine, acidify and extract with ether; evaporate, and apply the tests for picric acid to the residue—*e.g.*, dyeing wool yellow.

I repeated these observations on the 11th of April, and took again 6 grs. of picric acid, in 2 gr. doses, at intervals of some hours. At 9 30 p.m.—*i.e.*, seven hours after the second dose—the urine was of a distinctly orange colour. The colour deepened on the 12th.

The urine was acidified with H_2SO_4 and extracted with ether. The ethereal solution was evaporated to dryness, and then dissolved in water, and a small skein of white wool left in it. The wool was stained pale yellow.

Let me now turn to the main subject of my communication—viz., High Explosives.

The term "High Explosives" is of quite recent origin. The velocity of explosion in them is about 500 times greater than that of a "Low Explosive"—*e.g.*, gunpowder. It denotes compounds rich in N which, when detonated, furnish suddenly an enormous volume of heated gas, from 10,000 to 15,000 times the volume of the explosive. The amount of energy which will become available when

explosion occurs depends mainly upon the percentage of N in the compound. All explosives contain N, a singular fact when we call to mind the chemically and physiologically inert properties of ordinary free N, which forms, as we all know, about 80 per cent. of atmospheric air.

The first explosive prepared by man was gunpowder, some form of which was used in the battle of Crecy in 1346, when, as the old chronicle relates, the guns "threw little balls of iron to frighten the horses." For a long time the employment of gunpowder was limited to cannon and muskets, and nearly 200 years passed before it was utilised for small arms—*i.e.*, pistols.

After this momentous innovation in warfare the history of explosives is a blank for nearly 500 years, and no new explosive was invented.

The starting point of modern explosives was made by a Frenchman, Braconnot of Nancy, in 1832. He prepared explosive nitro-compounds by the action of HNO_3 on starch, and wood-fibres—*i.e.*, from carbohydrates.

In 1845, Schönbein, of Basle, discovered guncotton. A year later (1846) a most important advance was made by an Italian chemist, Ascanio Sobrero, who discovered nitroglycerin, or, as he called it, pyroglycerin. But it was not until 1863 that nitroglycerin was brought into the sphere of practical utility by a Swede, Alfred Nobel—the founder of the munificent Nobel prizes.

After many experiments he found that by mixing nitroglycerin with *Kieselguhr* (a siliceous infusorial earth) a plastic and absorbent product was obtained which was safer to handle and more manageable. This mixture soon became universally known as dynamite, or giant powder, as the Americans call it.

Millions of tons of dynamite were used in the con-

struction of the great Panama Canal. Likewise for the Suez Canal, and in the excavation of the Simplon and other long tunnels ; in mines, quarries, &c.

In 1905, the steamer *Chatham* sank in the Suez Canal. Among other things the ship was laden with 100 tons of dynamite. Divers went down and placed electric detonators in position. The gigantic explosion forced a jet of water to rise 1,500 feet in the air, and a hole 73 feet in depth was punched out of the canal bed.

Picric acid was discovered nearly 150 years ago, but it was not until 1867 that it was employed as a blasting powder mixed with $NaNO_3$ and K_2CrO_4.

A mixture of picric acid and collodion compressed and molten is known as melinite, or lyddite, and was extensively used in the Boer War.

About twenty years later (1886) an entirely new departure was put forward by a French chemist, as " smokeless powder," and soon after, Sir Frederick Abel and Sir James Dewar introduced us to cordite.

Cordite is a mixture of guncotton and nitroglycerin dissolved in acetone, and thickened by the addition of 5 per cent. of vaseline. It is largely used in British ordnance.

The outcome of these successive discoveries is that the old black gunpowder, which held its ground for more than six centuries, has been practically superseded in military service, and may, before long, become a matter of history only, at least in military warfare.

What are the sources of High Explosives ?

The answer is simple. They are derivatives of the two great groups into which most organic compounds fall. I refer to (I.) The carbohydrates (fatty : aliphatic group), and (II.) The hydrocarbons of the aromatic group.

The organic products employed as explosives are either (*a*) true nitrates, or (*b*) nitro-compounds.

The difference between them is important, and consists in this :—

In an organic nitrite or nitrate (true ester) the nitric (nitrous) radical is attached indirectly to the hydrocarbons nucleus through the medium of oxygen—

e.g., $RO.NO_2$ ($RO.NO$).
nitrate nitrite

In a nitro-compound the substituting NO_2 group is directly attached to the hydrocarbon radical—

e.g., RNO_2 ; *e.g.*, nitrobenzene, $C_6H_5NO_2$.

These nitro-compounds are more stable, and are not saponifiable by alkalies, whereas organic nitrates, being alcoholic salts or esters, are readily saponified.

$$R.O.NO_2 + KOH = K.O.NO_2 + R.OH \text{ (alcohol)}$$
$$C_2H_5ONO_2 + KOH = KO.NO_2 + C_2H_5OH.$$

Inorganic nitrates—*e.g.*, KNO_3 or NH_4NO_3—are often added in order by supplying oxygen to intensify the effect of high explosives. Similarly, $KClO_3$ or $KClO_4$ may serve, as an addition to one of the aromatic explosives—*e.g.* Rack-a-rock : cheddite.

Of the carbohydrate group, the chief individuals which concern us are cotton, or cellulose $(C_6H_{10}O_5)_n$ and glycerin $C_3H_5(OH)_3$

(*a*) By the graduated action of HNO_3 upon cellulose the degree of nitration can be so arranged as to yield products whose proportion of N ranges between 6 per cent. to 13.5 per cent. N. Pyroxylin, B. P. is Di-nitro-cellulose. Guncotton is Tri-nitro-cellulose. The term *nitro* is inapplicable to these bodies, for they are true nitrates—*i.e.*, esters. Practically, most violent propellants contain

nitro-cellulose (guncotton) wholly or partly gelatinised. All smokeless powders consist largely of nitro-cellulose in some form. Guncotton, even when moist, can be exploded by mercury fulminate just as readily as when it is dry. Used for charging torpedoes and sea-mines.

(*b*) Glycerin, by the action of concentrated HNO_3, is transformed into nitroglycerin, a heavy, oily-looking liquid. An alcoholic solution of nitroglycerin, 1 per cent., constitutes the Liquor Trinitrini B. P.

The term nitroglycerin is a chemical misnomer, for it has been shown that it is really a true ethereal nitrate $C_3H'_5'' (ONO_2)_3$, and is readily broken up by alkalies—*i.e.*, saponified like other esters.

Gelignite is a mixture of nitroglycerin, nitrocellulose, wood pulp, and KNO_3.

Most of us will remember that it was used by the rebels in the Dublin outbreak, April, 1916, and a quantity of it was found in the G.P.O.

Among the aromatic group we recognise three important parents of explosives, viz. :—

	Derivatives
Benzene (Benzol) C_6H_6	Trinitrobenzene, sometimes used for filling shells ; also trinitrocresol and trinitro-naphthalene Nitro-benzenes (mono-di-tri) *e.g.*, $C_6H_5NO_2$
Toluene (Toluol) $C_6H_5CH_3$ methyl-benzene	Tri-nitro-toluene T.N.T. $C_6H_2 (NO_2)_3 CH_3$
Phenol (carbolic acid) C_6H_5OH	Picric acid or Tri-nitro-phenol $C_6H_2 (NO_2)_3 OH$

Derivatives of aniline have also been used—*e.g.*, tetranitraniline and tetranitromethylaniline (tetryl.).

The metallic picrates are more explosive than the free acid.

Lead picrate detonates suddenly and violently when heated. (Experiment shown.)

Picric acid is the oldest organic dyestuff, and colours animal tissues and fibres, such as silk or wool, but not cotton.

This dye test is useful for the detection of picric acid in organic fluids. (Experiment shown.)

Although picric acid solution is somewhat extensively used as a dressing for burns I have not heard of any ill effects ensuing. The only harmful result I can find which appears to have been due to picric acid is a case of a severe burn on the foot of a young infant. It was dressed with a dusting powder, containing from 7 to 17 per cent. of the acid. This was followed by vomiting and diarrhœa. T. 100° F. ; P. 100 to 150. A general erythema developed, and the child died on the twenty second day after the accident.—Martindale, *Extra Pharm.*, 16th edit., p. 64.

Within recent times picric acid and picrates have receded in importance as explosive agents, and T.N.T. has come into the foreground, and has replaced picric acid as a filling for shells.

The reasons for the substitution are that T.N.T. has a lower M. P., is more stable, is insensitive to ordinary shock, and, being free from acidic properties, it does not attack metals or their oxides.

Furthermore, it is cheaper than picric acid, and is less poisonous.

T.N.T.

It is obtained by the graduated action of HNO_3 upon

toluene, and is a true nitro-compound. Occurs in small yellow prismatic crystals. Insoluble in water ; soluble in ether, acetone, and fixed oils. It is quite safe to handle. When heated in the open, it suddenly takes fire, without detonation. (Experiment shown.)

PHYSIOLOGICAL ACTION.

Absorption.—It can be absorbed by the skin, and is liable to cause dermatitis, followed by desquamation. Some persons are more susceptible than others. In my own case, I more than once rubbed in an ethereal solution on the back of my hand, and thereby caused slight punctiform erythema. I several times rubbed into the palms T.N.T. and liquid paraffin without result.

From observations of 1,100 workers Mr. Barnes considers that the main channel of absorption is the skin, especially if greasy, and the incidence of cases has been where the material was used, rather than where it was made.

Professor Benjamin Moore provoked toxic symptoms in himself by rubbing an oily preparation of T.N.T. into his palms. In a factory he had breathed fumes and dust systematically, but had not induced any symptoms of poisoning.

Digestive Troubles.—Munition workers who handle T.N.T. are liable to gastritis, abdominal pain, and persistent constipation.

Blood Changes.—T.N.T. absorption induces cyanosis and hæmolytic changes, and meta-hæmoglobin is demonstrable. The alterations in the blood are minutely described by Dr. M. J. Stewart in *The Lancet*, Jan. 27, 1917.

Liver.—T.N.T. brings about serious mischief in the liver. The lesion appears to lie somewhere between a subacute yellow atrophy and an ordinary multilobular cirrhosis of

irregular distribution. Jaundice appears, and ascites sometimes supervenes, and hæmorrhages occur in various situations, in both skin and viscera.

Urine is dark, and may contain albumen as well as bile. Crystals of leucin and tyrosin have also been observed.

In 1916 about 50 deaths had occurred from toxic jaundice, a mortality among those exposed of 0.05 per cent. When death occurs it usually takes place about three weeks after the first appearance of jaundice, and is preceded by delirium and coma.

TESTS FOR T.N.T.

(*a*) *In the Free State* (M. P. 80° C.).—A weak solution (ethereal) diluted with water strikes a deep red with KOH, preferably in alcoholic solution. (Experiment shown.) Di-nitro-toluene yields a blue colour with KOH.

(*b*) *In the Urine.*—T.N.T. may be recovered unchanged from the fæces, but does not exist in the free state in the urine, except, perhaps, in minute amount. To detect it in urine, Webster's test is convenient (*Lancet*, Dec. 16, 1916). Add to urine (12.5 cc.) an equal volume of dilute H_2SO_4. Pour the mixture into a separating funnel. Add about half its volume (10 cc.) of commercial ether. Shake up well and allow to settle. After a few minutes draw off the lower stratum of urine and acid. Now add about twice its volume (25 cc.) of tap-water to the ethereal solution, shake up again, and, after settling, run off the water. Finally, let the ethereal solution flow into a test tube and apply the KOH test, which at once strikes a bright red. (*Lancet*, Dec. 16, 1916).

In order to verify this test I made some experiments on myself.

On Sunday, March 18, I took three doses of 1 gr. each, of

T.N.T., with a little sugar, at intervals of some hours—viz., 11.15 a.m., 1.5 p.m., and 6.30 p.m.

That night and next morning I collected some urine and tested the mixed specimen as above.

The urine was high-coloured, almost orange tint ; acid in reaction, free from albumen, and gave no reaction with the iodine and chloroform test for bile. The ethereal extract from the acidified urine gave a bright red with the alcoholic solution of KOH.

The fresh urine, before treatment with H_2SO_4, gave scarcely any reaction with KOH ; very slightly deepened in colour, and slowly showed a pale red. A little T.N.T. added to normal urine reacts at once to the potash test, applied directly, or to the ethereal extract.

I repeated these observations yesterday, April 12, and took three doses of T.N.T., 1 gr. each, at 4.30 p.m., 9.30 p.m. and 10.30 p.m., with the same results.

Dr. Nesbitt thanked Professor Smith for the valuable information that he had given the section on a subject of great importance, and about which it was difficult to obtain accurate knowledge.

Dr. W. M. Crofton described the symptoms presented by several patients who had been accidentally poisoned by cordite fumes. He inquired whether any satisfactory treatment had been found for persons suffering from poisoning from the fumes of high explosives.

Dr. Law stated that soldiers sometime produce in themselves disordered action of the heart by eating cordite.

Dr. Smith, in reply, stated that he had no personal experience of the treatment of persons suffering from T. N. T. poisoning.

BURNS AND SCALDS AS A CAUSE OF CHILD DISABLEMENT AND DEATH.

By SIR JOHN MOORE, M.A., M.D., M.Ch., D.P.H. Dubl.; D.Sc. (*Hon. Causâ*), Oxon.; F.R.C.P.I.;

Honorary Physician to H. M. the King in Ireland;
Senior Physician to the Meath Hospital and County Dublin Infirmary.

[Read in the Section of Medicine, April 13, 1917.]

Just five years ago—on Friday, April 12, 1912, I read a paper before this section of the Academy on "Non-notifiable Diseases as a Cause of Mortality in Childhood, with Suggestions for their Control."

The supreme value of child-life at the present grave crisis in the history of the British Empire is recognised by all thinking men and women of our country and our race. How to safeguard and preserve it, therefore, becomes a question of national importance. It has occurred to me that in endeavouring to answer that momentous question we may lose sight of a cause of disablement and death to young children which is absolutely preventable, and which accordingly should be prevented.

The cause to which I refer is burning or scalding. Sir William J. Thompson, M.D., the Registrar-General for

Ireland, has kindly furnished me with the following official figures :—

"REGISTRATION OF DEATHS, IRELAND.

"Statement showing the number of deaths from Burns or Scalds (distinguishing deaths of children under five years of age) registered in the Dublin Registration Area during each of the five years 1912-1916. The figures have been extracted from the Registrar-General's Yearly Summaries of the Weekly Returns.

	Deaths from Burns or Scalds	
Year	Persons at all ages	Children under 5 years
1912 ...	47 ...	25
1913 ...	42 ...	19
1914 ...	50 ...	28
1915 ...	53 ...	29
1916 ...	35 ...	22
Totals for 5 years ...	227 ...	123
Percentages ...	100 ...	54.2

"WILLIAM J. THOMPSON,
"*Registrar-General.*

"General Register Office,
"Charlemont House, Dublin,
"15*th March*, 1917."

The National Society for the Prevention of Cruelty to Children in 1911 issued a leaflet headed "Death by Burning : a Warning." That leaflet contains these sentences : "In the ten years, 1900-09, 2,166 children under five years of age were burned or scalded to death in Ireland. During 1909 alone, 214 children so met their death.

At my request the Staff Sister of the Surgical Landing at the Meath Hospital and County Dublin Infirmary prepared a list of the admissions, during the hospital year

ended March 31, 1917, to the Smyly Children's Ward, of burned or scalded children. The casualties were 12 in number. I give the sex, age, nature of the injury, and result of the accident in the case of each of the twelve patients :—

1. Girl, aged 10 years, admitted April 5, 1916; died next day, 27 hours after admission. Verdict at Coroner's inquest : "Accidental Death."

2. Girl, aged 3 years, soldier's child, admitted April 13, for slight scald of arm; discharged May 17.

3. Girl, aged 2 years, soldier's child, admitted June 6; died June 8, 15 hours after admission. Verdict at Coroner's inquest : "Accidental Death."

4. Boy, aged 9 months, admitted July 17; discharged July 26; scald of neck and slight scald of arm.

5. Girl, aged 16 months, admitted August 20; discharged October 11.

6. Girl, aged 1 year, admitted August 20; discharged September 24.

7. Boy, aged $3\frac{1}{4}$ years, admitted October 12; discharged November 17.

8. Girl, aged 1 year, admitted for scalds, November 11; discharged November 30. A soldier's child.

9. Girl, aged $3\frac{3}{4}$ years, admitted for scalds, November 20; discharged November 28.

10. Boy, aged 3 years, a soldier's child, admitted December 15, 1916; discharged January 20, 1917.

11. Girl, aged 3 years, admitted December 16, 1916; discharged January 12, 1917.

12. Girl, aged 3 years, admitted February 21, 1917; died next day, within 24 hours of her admission.

The lesions in these cases were chiefly burns of the head, with severe injuries to the eyes, and scalds of the neck and arms—the latter caused by the child upsetting a teapot of hot tea, or a plate of hot porridge, or a bowl

of hot soup. The burning cases were caused by the children's clothes catching fire.

It will be noticed that nine of the twelve children were girls. Also that four of the sufferers were children of soldiers—a fact which suggests a misuse of the mothers' separation allowances. Three of the twelve children died —a mortality of 25 per cent.

Now it is to be remembered that the foregoing figures relate to only one of the ten clinical hospitals in Dublin, among which the National Children's Hospital, Harcourt Street, and Temple Street Children's Hospital are not included. If we take these Meath Hospital figures as a fair average, it is obvious that the accidents by burns and scalds among children of very tender age in Dublin are both many and fatal.

Apart from the loss of life caused by these preventable accidents, it is painful to think of the suffering—amounting to agony—through which these child-patients have to pass for weeks or months before they recover. And even then they are probably disfigured for life.

What are the remedies for this lamentable tale of suffering and death? In the leaflet of the National Society for the Prevention of Cruelty to Children there are some self-evident propositions which will bear quotation. They are as follow :—

"It is the duty of parents to protect their children."

"It is the duty of the Society to deal with parents who fail to do this."

"Small children should never be left alone in a room with an unguarded fire. The risk of danger is very great. Children are fond of playing with fire, and have no idea of the risks they run."

The pamphlet goes on to show that most of the 214 children who in Ireland met their deaths in 1909 by burns

or scalds would not have lost their lives had a fire-guard been provided. A fire-guard is an inexpensive article.

It is now an indictable offence to leave a child in a room where there is an unguarded fire. In proof of this last statement, it may be well to quote *in extenso* Section 15 of the Children Act, 1908 [8 Edw. 7, Ch. 67], which runs as follows :—

"If any person over the age of sixteen years who has the custody, charge, or care of any child under the age of seven years allows that child to be in any room containing an open fire-grate not sufficiently protected to guard against the risk of the child being burnt or scalded, without taking reasonable precautions against that risk, and by reason thereof the child is killed or suffers serious injury, he shall on summary conviction be liable to a fine not exceeding ten pounds : Provided that this section shall not, nor shall any proceedings taken thereunder, affect any liability of any such person to be proceeded against by indictment for any indictable offence."

Fortunately, this Act of Parliament, which has been well called "The Children's Charter," applies to the whole of the United Kingdom. In its application to Ireland the measure is subject to various modifications—twenty in all—which are set out in detail in Section 133 of the Act.

Unlike many, if not most, enactments by the Legislature, Section 15 of the Children Act, 1908, is expressed in clear language, easily understanded of the people. It is the more to be regretted, therefore, that effect has seldom, if ever, been given to the salutary preventive procedure contemplated under the section. Personally, I am unaware of any prosecution having been instituted under its provisions since section 15 became part of the law of the land in 1908—nine years ago.

I have reason to know, also, that His Majesty's Coroners have not always considered it necessary to require a rider to the verdict of the jury in fatal cases of burning or scalding in young children. In two fatal cases of burns in children of tender years treated at the Meath Hospital during the past year, the verdict at the Coroner's inquest was "Accidental Death"—a self-evident proposition, which scarcely required the majesty of Coroner's Law for its proof. The question which remains unanswered is: "What reference, if any, was made at the inquiry to Section 15 of the Children Act, 1908?"

Children are, as I have already stated, the most valuable asset to the Empire and the Race at the present time. Child-life should, therefore, be most jealously guarded against harm, not only from without, but especially from within, the home.

In one of his most beautiful and touching lyrics, the poet Longfellow caught this idea. I am tempted to quote some of his lines:—

O child! O new born denizen
Of life's great city! on thy head
The glory of the morn is shed
Like a celestial benison!
Here at the portal thou dost stand
And with thy little hand
Thou openest the mysterious gate
Into the future's undiscovered land.

* * *

Like the new moon thy life appears;
A little strip of silver light,
And widening outward into night
The shadowy disk of future years.

Dr. Law stated that in the colonies in which he had worked deaths of children were more common than they are at home.

Dr. Rowlette stated that in his opinion the only possible

way of lessening this cause of child death was to form a public opinion which would compel people to adopt the precautions recommended, and induce the Government authorities to inflict the statutory penalties on those who neglect to do so.

Dr. Nesbitt expressed the opinion that the accidents were due more to the wholly unsuitable conditions in which the poor people live than to the carelessness of the parents.

The President also pointed out the difficulties of carrying out the provisions of the Children's Act under the conditions that exist in a city like Dublin.

NOTES OF A CASE OF MELÆNA AND HÆMATEMESIS NEONATORUM.

By BETHEL SOLOMONS M.D., F.R.C.P.I.;
Gynæcologist to Mercer's Hospital, Dublin.

[Read in the Section of Obstetrics, April 20, 1917.]

In this very fatal condition the details of a successful treatment are useful, and for that reason I submit the notes of a case which occurred in my practice.

The mother was a young primipara, and delivered herself of a full-term female—weight, $8\frac{1}{4}$ lbs.; length, 22 inches—after 7 hours' labour, on March 16th, at 8 p.m. The family history was good on both paternal and maternal sides.

I saw the child on March 17th in the morning, and everything was normal. I then left town for 24 hours, and did not visit the case again until the following evening. On March 17, at 5.30 p.m., the baby vomited a little brown fluid. For the next three hours she was very restless. At 9.30 she passed a large tarry motion with streaks of blood through it; at 10.15 she vomited about 3 ounces of red blood. From 10.30 until 11.0 she was very collapsed, with lips and fingers cyanosed; at 11.30 she passed a very large bloody offensive motion. She seemed in pain, and did not sleep more than ten minutes at a time. On the morning of the 18th, as I was away, Dr. Fitzgibbon kindly saw the patient for me. At 9 a.m. she had a very large fluid motion with blood through it. At mid-day, horse serum, 2 c.cs., was injected into the loin. Very shortly after this there was a motion containing a large quantity of blood. Some

blood was vomited. I saw her at 5.50 p.m., and soon after this there was a large stool. At 10 a.m., horse serum, 4 c.cs. was injected. On this day she had four doses of a mixture containing in each dose one grain of calcium lactate, and one minim of a one in a thousand solution of adrenalin chloride. She had a restless night, waking every hour.

March 19th.—At 3.15 a.m. and 4 p.m. large tarry motions were passed. She looked better, and I decided to wait before giving any more horse serum. However, after the first tarry stool I injected 5 c.cs. hypodermically. The adrenalin mixture was administered 12 times. She had a good night, and slept well.

March 20th.—Slight melæna twice—at 3 a.m. and 4 p.m. At 4.45 p.m. 5 c.cs. of horse serum hypodermically, 12 doses of the mixture in the 24 hours. She was very restless, and evidently in great pain, which was probably due to flatus.

March 21st.—In much pain. At 2 a.m. a small rubber catheter was passed per rectum, and a large quantity of flatus came away. The chill was taken off the water, which until this day had been given dead cold. There was one small tarry stool.

March 22nd.—At 2.20 a.m. the infant was very restless, and inclined to cyanosis, and looked as though about to die. Horse serum, 5 c.cs. was given by mouth; this was repeated at 9.30 a.m. She seemed weak through the day, and I decided to give her something stronger than water. I therefore ordered albumin water (one egg to the pint of water) every two hours, which was the first "food" given. This was the sixth day of the illness and the seventh since birth. The mixture was given eight times in the day.

March 23rd.—Albumen water 2 ounces, every two

hours. Mixture, eight times. Horse serum, 5 c.cs., twice in the 24 hours by mouth. Two motions; improved, but still rather tarry. One pound weight lost in the week.

March 24th.—Albumen water during the night. At 8.30 a.m. the first slightly yellow motion. At 11 a.m., rennet whey 2 ounces; serum, 5 c.cs. by mouth twice. Two more yellow motions. Rennet whey through the day. The mixture three times.

March 25th.—The mother's breasts had been kept pumped in order to continue secretion, and on this day the baby had 2 drachms of breast milk with rennet whey. Serum, 5 c.cs. by mouth; mixture three times in the day; two motions.

March 26th.—Whey and breast milk; mixture three times daily.

March 27th.—Breast feeding only.

After this the baby gradually improved, and is quite healthy to-day. Many theories have been brought forward to account for intestinal bleeding in the new-born, and I do not intend to enter into a discussion of them here. I believe it possible that one of the unknown toxæmias of pregnancy which circulate in the mother is carried to the child; whether causing a premature gastric secretion, as suggested by Dr. Crawford, or in what other way the ulceration is caused, it is impossible with our present knowledge to state. The very large *offensive* motion which was passed on the first day of the illness was strong evidence for a toxæmic theory. In *post-mortems* on eclamptic patients hæmorrhages have been found in various intra-abdominal sites. When eclamptic fits occur in infants, when scopolamine can be demonstrated in the urine of the new-born, why should not gastro-intestinal hæmorrhage be caused by a toxæmia carried from the mother to the

child while still *in utero*? The starvation treatment, which is so successful in cases of maternal toxæmia, is extraordinarily successful in a case such as the one described.

The following are the points which struck me most forcibly :—

1. That the child was female : the condition being supposedly more common in males.

2. The extreme vitality of the child and the amount of blood which she could lose and still live.

3. The fact that ice cold water, which is recommended in these cases, causes great pain ; while, when the chill is removed, hæmorrhage does not recommence, and there is no more pain.

4. That starvation can be sustained for so many days (six in my case).

5. That the cure in this case was effected by good nursing, warmth, quiet, water, the mixture, and the injection of serum. That the baby disimproved when the serum was temporarily stopped, and improved when administered again. That large doses of serum give better results than small, and that, when given hypodermically or by mouth, the result seems to be the same. That while no anaphylaxis was noticeable in my case, the physician should always be ready for this phenomenon, for which the best treatment is pituitary extract in small doses.

The serum used was B. and W. Normal Serum, No. 1.

Dr. Tweedy said these cases nearly always are due to duodenal ulcer, and horse serum in massive doses appears to act as a specific. The dose should not be smaller than 10 c.cs. It is seldom necessary to repeat the dose. Once the bleeding ceases the infant's condition rapidly improves,

and perforation of the bowel never appears to occur. Starvation is unnecessary for more than two days; breast milk may then be given.

Dr. Sheill said he was particularly interested in this case, as he published notes of a similar one about two years ago, and his treatment by horse serum was the first recorded success. He disagreed with Dr. Solomons regarding the starvation of the child for six days, and he believed a more rapid and successful result would have been obtained—as it was in his own case—by 10 c.c. doses of serum, instead of the smaller and repeated doses; and, moreover, the grave risk of anaphylaxis would be avoided.

Dr. Crofton said he thought the treatment admirable, and discussed the mechanism of the action of the serum. He thought it must act by inducing a larger production of thrombin, since, if mixed with fresh blood outside the body, it delayed clotting. He described two cases of anaphylaxis occurring after the first injection of serum intravenously, and discussed the probable cause of the change in the serum producing this. These cases had led him to introduce pituitrin for the treatment of anaphylaxis. The only criticism he had to offer of Dr. Solomons treatment was the exhibition of lime salts, which could have been better given in the form of the mother's milk.

Dr. Solomons, in reply, said that gastro-intestinal hæmorrhage in the new-born was caused by many other factors than duodenal ulcer—*e.g.*, intussusception, &c. He thought a toxæmic theory likely, and cited *post-mortems* on eclamptic patients where the very evident cause of death was intra-abdominal bleeding. There were other symptoms present which led him to this idea. He thought, therefore, that starvation was an important part of the treatment, as it was in the successful treatment of the toxæmias of pregnancy. He did not think that too much can be learned from one case, and preferred 5 c.cs. in repeated doses to the dose in the case reported by Dr. Sheill. Although Henoch is an ancient authority, it was he who recommended ice-cold water, but he (Dr. Solomons) had the chill removed.

THE VAGINAL PLUG.

By E. HASTINGS TWEEDY, F.R.C.P.I.;
Acting Master, Rotunda Hospital, Dublin.

[Read in the Section of Obstetrics, April 20, 1917.]

The treatment of ante-partum hæmorrhage by means of a vaginal plug is one of the oldest devices of surgery, yet it is within recent years only that the method of applying the plug has been properly understood. I remember hearing, when a student, my lecturer on obstetrics assert that he knew of no more efficient material for plugging than his silk pocket-handkerchief, and to illustrate his contention he took from his pocket an enormous scarf, dull red in colour and stained deeply by snuff and nasal secretion. I remember also an elderly doctor asserting that the family bath sponge made a most efficient vaginal plug. In my early professional days it was considered dangerous to introduce several small plugs lest some of them should be subsequently left in the vagina. It was to guard against this danger that the ridiculous kite-tailed arrangement was devised.

The subject of vaginal plugging was, however, seriously discussed only by the antiquated obstetrician, for the then modern one considered the method to be the most unsuccessful of futile manœuvres. At the present time one can hardly appreciate how great was the moral courage that enabled Sir William Smyly to become its advocate, and how keen and resentful was the opposition that his advocacy of it induced. The controversy has long since

been forgotten, and statistics have proved that in its efficiency the plug has at present no rival.

The method as now practised depends for its efficiency on the capacity of the plug to push the lateral fornices of the vagina far up into the abdomen, so that they can be felt by abdominal palpation as hard-rounded tumours situate well above the level of Poupart's ligaments. To plug efficiently the left hand should be passed into the vagina with the palmar surface directed towards the hollow of the sacrum, while the tips of the fingers lie behind the cervix. Small pieces of cotton-wool, squeezed out of lysol solution and each the size of the thumb's knuckle, are then taken and inserted by means of the right hand round the cervix. The fingers of the left hand are kept busy squeezing the pellets into a compact mass and forcing the spaces between them to permit the insertion of still another plug. This process is continued in a systematic manner from above downwards till the vulva is reached and the vagina can hold no more. A T-bandage is applied to keep the plug in position, and an abdominal binder is fastened tightly from above downwards to press the side walls of the uterus against the vaginal dam, and thus completes the operation. A plug so applied will cause immediate cessation of hæmorrhage, and when it is removed after the lapse of hours so much blood only will be found as can be accounted for by the flow that took place during the operation.

Various explanations have been given of how the plug acts, and many of these by their ineptitude have greatly prejudiced the method of treatment. Many suppose that a back pressure on the blood-vessels is caused by the out-poured blood above the plug. This explanation is quite inadequate, and justifies Berkeley and Bonney in writing

that "on first consideration the logic of plugging the vagina to arrest bleeding that is going on some distance above it may not appear obvious, yet its efficacy in cases with free external hæmorrhage is proven." These writers satisfy themselves that the efficacy of the plug depends on it increasing the power of the uterus to contract. When dealing with concealed accidental hæmorrhage, however, with the accompanying uterine inertia, they are driven to express a doubt whether the plug can have any directly useful effect. They say "it is easy to apply" and that it "has this great advantage, that it avoids rapid emptying of the uterus, a proceeding which always impresses severe shock on these patients. We think there can be no doubt that in some of these cases the bleeding has stopped by the time the practitioner comes on the scene, and that in such plugging the vagina has the great advantage of compelling him to pursue a course of masterly inactivity."

No doubt a plug applied in the method recommended by these writers will produce very different results from one applied in the way described above. They apply the plug with the aid of a speculum and a forceps, and use for this purpose rolls of cotton, each tied round with a string to facilitate removal. I feel sure it would not be possible to place in position one dozen of such rolls as are portrayed in their illustration. The plug, as I understand it, should consist of from 20 to 40 separate pieces, packed and dovetailed so closely into each other that they could not possibly be removed by pulling on them with a string.

I have quoted from the work of these writers to show how much, even now, the correct action of this device is misunderstood, and how much the method of treatment has suffered in consequence.

In 1898 I laid before this Section of the Academy evi-

dence to show that the plug acted directly as a compressor of the uterine vessels. Recently I have re-read that article and still cannot understand how anyone who took the trouble to weigh carefully the evidence I then adduced could have any doubt as to the truth of my contention. In the report of the discussion on that paper I find that, with the exception of Dr. Doyle, a general practitioner, all the speakers either ignored the suggestion altogether or treated it with contempt. It is therefore not surprising that the suggestion has not received general recognition in other countries. If truth could be destroyed by means of a studied boycott, assuredly this one would have shared that fate.

After nearly twenty years I am able now to bring forward positive proof that the plug acts in the way I have suggested. During the performance of several Cæsarean sections, after the delivery of the child and the stitching of the uterine wound, I have got an assistant to pass his hand into the vagina and to press it into the lateral fornix. In each instance his hand has formed a prominent convex tumour, which can be seen from the abdominal wound to press the base of the broad ligament upwards above the level of Poupart's ligament. On this tumour the uterine vessels lie, and I have been able to demonstrate to my colleagues, Sir William Smyly, Dr. Purefoy, and my Assistant Masters, that the pulsation in these vessels is stopped by the pressure. On the last occasion I asked my assistant to tell the class what he felt. First, he said the pulsation in the artery had ceased, and when I pressed the fundus of the uterus downwards to reproduce the conditions induced by the abdominal binder, he said his finger had become nipped between the resistance below and the side wall of the uterus. If we remember the size and

importance of the blood vessels, both ovarian and uterine, which are situate in this region, the result of direct pressure exercised on them by a tightly fitting vaginal plug will at once become apparent.

In labour the opening of the internal os removes from the lateral fornices the support that it formerly afforded, and so allows the fornices to be pushed far up into the abdomen. In the non-pregnant state this cannot be done on account of the tightness of Mackenrodt's ligaments. They are under the control of the uterine muscle fibres. It is the uterus by its inherent power, acting through these so-called ligaments and its other tendons, that forms the true floor of the pelvis. This is another truth of vital interest to the gynæcologist which I trust will not in its turn lie dormant for twenty years.

The vaginal plug is not easy to apply nor is its application harmless. Sometimes, in spite of our best efforts, we cannot at the first attempt insert sufficient material to stop the bleeding. In such a case the plug must be removed entirely and then reinserted. On the second attempt the procedure will be found to have become much more easy on account of the dilatation of the vagina. Pain, distress, and some shock always follow from the application of the plug, and superficial tearing of the mucous membrane of the vagina is almost certain to occur.

It is clear that the words "vaginal plug" mean, in Dublin, something different from what they do elsewhere, and when one criticises the method of treatment one must bear the true meaning in mind.

Sir William Smyly said that he considered Dr. Tweedy's paper of very great practical importance, and not the less so because the views expressed in it were not in harmony with

those generally held by obstetric writers. When he (Sir William Smyly) became Master of the Rotunda Hospital version and extraction were generally accepted as the only rational treatment in cases of severe accidental hæmorrhage, but the results were so bad that he abandoned it. Rupture of the membranes no doubt succeeded in the majority of cases. but altogether failed in some of the worst. He then resorted to plugging the vagina, and that treatment had been adhered to by all succeeding Masters, and the Reports of the Hospital were sufficient proof of its success. In his paper Dr. Tweedy had very confidently affirmed that the plug does control the hæmorrhage, and that it does so by compressing the uterine arteries, which at term are very closely related with the lateral fornices of the vagina. In both these statements Sir W. Smyly agreed with him, and could confirm his observations as to the control of the circulation in those vessels by pressure from the vagina during Cæsarean section. He did not, however, see that that disproved the older explanation: both might be, and probably were, true. In many cases in which no symptoms of hæmorrhage had been noted, evidences were to be found in the placenta after birth of former limited hæmorrhages. What was the explanation of the limitation and control of such hæmorrhages if it were not that the capacity of the spaces into which it occurred were limited, and when the tension became equal to the blood pressure in the sinuses the hæmorrhage was checked? When he met in works on obstetrics with statements as to the futility of plugging he always sought the author's directions, and generally found that gauze or dry cotton was recommended for the purpose. Such plugs would, no doubt, be useless, and would soon be found quite loose in the vagina. The method described by Dr. Tweedy, and which he had learned from him, was efficient. Concealed internal hæmorrhages were altogether different from the cases of external hæmorrhage: they were generally, if not always, associated with eclampsia or other toxæmias, and the effusion would be found as often in the peritoneal cavity as in the uterus; or in both. Abdominal section was the treatment in such cases.

Dr. Sheill suggested that the fist-in-the-vagina method

would be of great service during Cæsarean section as a hæmostatic during placental removal and uterine suture.

Dr. Bethel Solomons said that he had found the treatment of external accidental hæmorrhage by vaginal plugging infallible. He had been called to cases where the plug had failed to stop the hæmorrhage, but he always found that bad technique was the cause. While he approved of endeavouring to find how the plug stopped the hæmorrhage, he hoped that vaginal manipulation in cases of Cæsarean section would not be persisted in, as these manipulations converted a comparatively safe operation into a dangerous one. Concealed hæmorrhage must be treated by laparotomy.

Dr. Barry said he discarded gloves when plugging the vagina, as he had found it difficult to plug satisfactorily if he wore them.

THE INFLUENCE OF ENVIRONMENT ON THE MORPHOLOGY OF THE ACNE BACILLUS.

By W. M. CROFTON, M.D.;
Lecturer in Special Pathology, U. C. Dublin;
Visiting Physician, Royal National Hospital for Consumption in Ireland;
Pathologist, Dr. Steevens' Hospital;
Late Temp. Capt., R.A.M.C.

[Read in the Section of Pathology, April 27, 1917.]

Owing to the rapidity with which one generation succeeds another schizomycetes form excellent material for the study of the influence of environment on morphology and method of life.

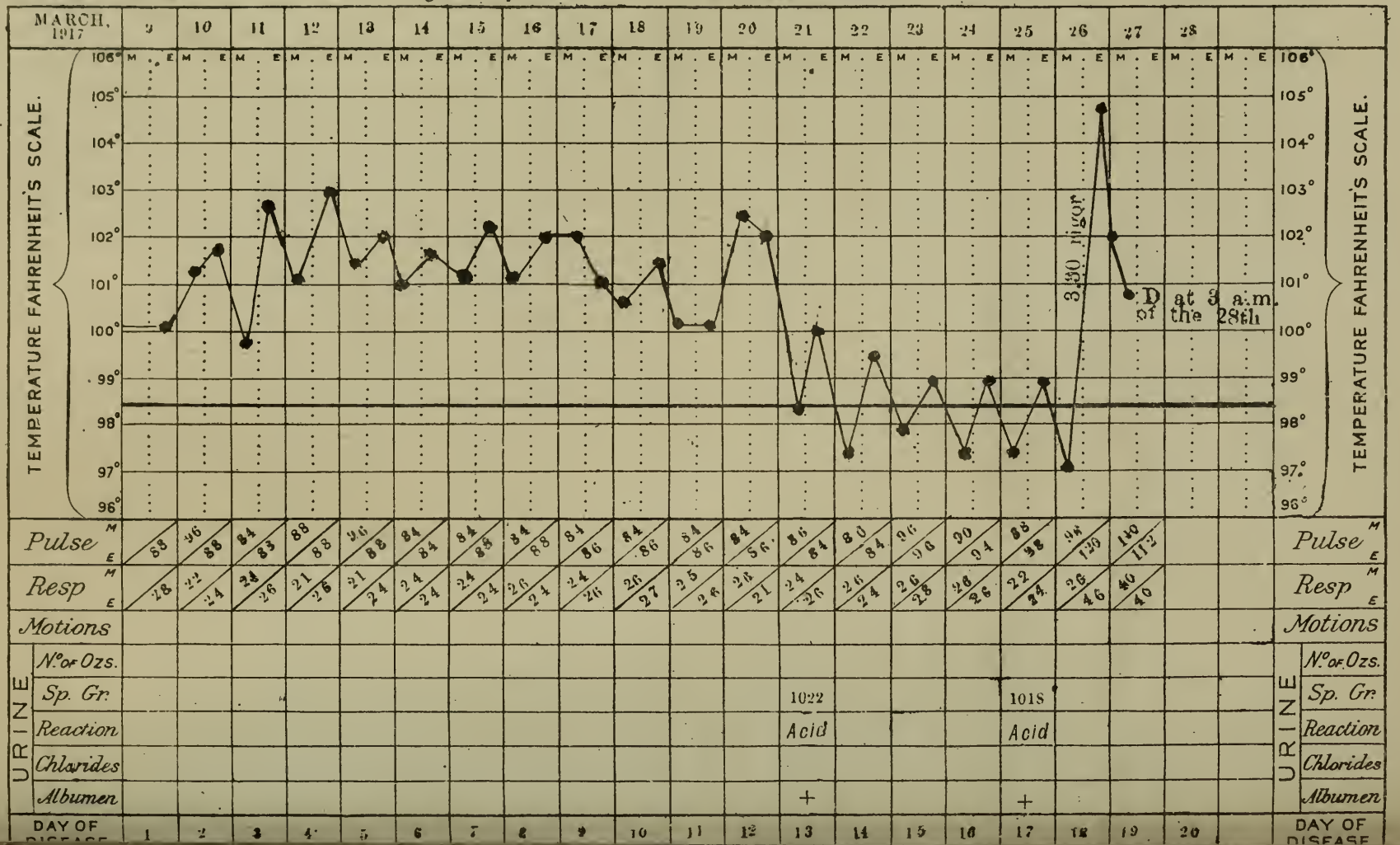
The decrease of toxicity (*i.e.*, capacity for breaking down living protoplasm) of a microbe when made to lead a saprophytic existence and the increase of toxicity on passage through a series of susceptible animals illustrate well the influence of environment on mode of life.

The survival of the fittest can be studied by growing a strain of microbes on a series of culture tubes containing increasing quantities of some inhibitory substance. As the amount of inhibitory substance increases fewer and fewer colonies grow, only the especially vigorous microbes continuing to be able to survive and to reproduce themselves. The influence of environment on morphology is well illustrated by the acne bacillus.

The acne bacillus, as seen in smears of sebaceous

CLINICAL CHART OF TEMPERATURE, &c.

Name S. M. *Age* 87 years. *Disease* NEPHRITIS AFTER BURNS. *Result* DEATH.



material from the skin, is a short, plump Gram-positive bacillus.

Some years ago I made slope cultures of such material on 2 per cent. glucose-agar and serum-agar and incubated them anaërobically. On the glucose-agar colonies of Gram-positive bacilli grew and also colonies of staphylococci; on the serum-agar only staphylococci grew. It struck me that it was possible that the medium might have had some influence on the shape of the bacillus, that some of the colonies on the serum medium might be really those of short acne bacilli which looked like cocci. I therefore made a culture of the bacillus from the glucose-agar on to the serum-agar and incubated anaërobically. Only cocci grew. These cocci were then transferred to glucose-agar and only bacilli grew. The experiment was repeated several times, so that there could be no doubt about the matter. Both mediums, of course, grow staphylococci equally well. If the sebaceous material is sown on + 30 agar slopes and incubated anaërobically the growth of the skin staphylococci will, as a rule, be completely inhibited if the oxygen has been sufficiently removed. Towards the end of a week small colonies appear which increase in size until at the end of three weeks they are several millimetres in diameter. Their appearance is quite characteristic. When typical they have the shape of the male nipple, buff-coloured in the centre, shading off to a creamy brown towards the periphery. If these cultures are now incubated aërobically, the colonies increase rapidly in size, but the new growth consists entirely of cocci surrounding the bacilli like a halo, or if the bacilli are subcultured on to + 30 or + 10 agar slopes and grown aërobically they grow as cocci, the colonies being indistinguishable from those of Staphylococcus albus

as are the cocci themselves. If the Gram-positive bacilli are subcultured on to + 30 agar and grown anaërobically they sometimes grow as Gram-negative bacilli, these also grow as Gram-positive cocci if grown aërobically. Even if grown anaërobically they will grow as cocci on + 10 agar. So that it is evident that both anaërobic conditions and a suitable medium are necessary for the development of the bacillary form.

The alternative possibility that staphylococci might survive in the acne bacillus colonies on the + 30 agar and develop under aërobic conditions is ruled out of court by the glucose-agar serum-agar experiments and by the fact that when the colonies are composed of Gram-negative bacilli no Gram-positive cocci could be seen on staining.

The method of growing on + 30 agar makes the isolation of the bacillus easy and certain. This medium should contain 3 per cent. of agar as the acidity makes the medium too soft if the usual amount is used. If the broth is made from heart muscle the medium is usually from + 20 to + 27, so that only a little extra acid need be added.

SHELL-SHOCK.

By MAJOR F. C. PURSER, M.D., Univ. Dubl.; F.R.C.P.I.;

Physician to Mercer's Hospital, Dublin.

[Read in the Section of Medicine, May 11, 1917.]

Shell-shock, as a name covering a multitude of ideas, has been in everyone's mouth for two years past and more, and I have thought that some account of the conditions which the term is stretched to cover would be of interest to the Academy. I have not tried to draw up precise statistics of the frequency of this symptom or of that among the very many cases I have seen, my wish is rather to give a general account of the manifestations and progress of the condition as I have found it.

Men who have been exposed to the force of high explosive shells or who have been blown up by mines are liable to suffer from a very great number and variety of symptoms. They may be divided into three main groups. One group, which may be called shell-shock proper, in which the grouping of symptoms is new to us; and a second group of cases, of traumatic neuroses, of which no manifestation has been recorded which was not recognised and recorded before the war. The third group is one where there is absolute mental alienation. I do not intend to deal with cases where the sufferers became actually insane, for I have had nothing to do with these cases, nor do I intend to refer to the cases of traumatic neurosis, for to do so would add insupportably to the length of my communication. The most pitiable cases I have seen were in men who had been buried in mine explosions, and it is my

impression that being buried adds to the intensity of the trouble. How it all starts exactly is often very doubtful. Many of the men know of the origin of their trouble only by hearsay: they remember, perhaps, an explosion, and then a blank, which may represent any length of time of unconsciousness. Some do not develop symptoms until some days after the shock, this especially among the cases of traumatic neuroses (and these cases seem often for a while to become progressively worse). Others wake up to consciousness and misery, which as a rule becomes steadily less.

English, Scotch, or Irish seem all alike susceptible. Nationality makes no difference. But age does, and there are unquestionably more cases between the ages of twenty and twenty-eight than between twenty-eight and forty-five. The bulk of the sufferers, especially the sufferers from shell-shock proper (as I have called it), are of poor physique and apparent powers of endurance.

As the cases of traumatic neurosis show an endless variety of symptoms of paralyses and contractures and sensory phenomena, so the cases of shell-shock proper show an extraordinary similarity of symptoms. They are:—

1. *Headache.*—It may be in any part of the head, but is usually occipital or "behind the eyes." It may be constant with intermittent exacerbations, or intermittent with periods of complete freedom. It is often associated with a feeling of "dizziness" or giddiness.

2. The feeling of "dizziness" may amount to severe vertigo and cause the patient to fall. But usually it is not so severe, and in the majority of cases I believe it to be not giddiness or vertigo at all, but a state of mental confusion. In some cases it comes on only when an effort

is being made—such an effort as speaking to a strange officer. In no case have I noted nystagmus, not even when the patient confessed to the feeling of dizziness coming on during my examination of him.

3. *Insomnia.*—This has been confirmed by nurses, orderlies, and other patients. The man lies awake "for no reason," or because of the least noise, or because his head aches more at night. Sometimes he sleeps, but is frequently awakened by bad dreams.

4. Bad dreams are constantly complained of. They are red, deafening and choking—nearly always about the war. Or else about family tragedies—invariably about something unpleasant. Often the man awakes with cries and struggles of terror. Cessation of bad dreams may be looked on as a favourable sign, for they appear to cease once the patient's condition begins definitely to improve.

5. *Frequent pulse.*—This varies up to 140 per minute, and I have seen one or two cases where it was over 160. In about 30 cases I have measured the blood-pressure. It varies greatly. I have found it raised, and in other cases not raised. It has rarely been over 150 m.m. Hg.

6. *Tremor.*—This may be coarse and general, or fine and general. Usually it is fine, about 6 to 8 to the second, and in the hands. I do not think observation makes it worse.

7. These symptoms represent but half the case of shell-shock. There is through and through them all a something less tangible but quite definite. It is a state of depression, mental and physical; a state of silent, hopeless inability. (And one must leave oneself open to feel this. It saves one from making harsh misjudgments, and it protects one from fraud. For sufferers from shell-shock have their imitators, and headache and bad dreams and

dizziness are easy symptoms to feign. But the alert readiness to help, the energetic muscles in the back, brisk standing to attention, and communicativeness are all foreign to the genuine sufferers.) They are not reminiscent. Nothing upsets them so much as being questioned about or reminded of their experiences. They hate a noise, they hate bustle, and they shun a company. They also suffer greatly from the cold. They are happiest, and their condition improves most quickly in circumstances where they are warm and quiet and not bustled.

As a rule these cases do well. After six months or less they are fairly normal and can be profitably employed. But I know of no way of judging, at the outset, how any given case will do. I believe more reliance can be placed on the mental condition of a man's stock (if it be discoverable) than on any of his own symptoms or want of symptoms.

The physical and mental state which I have tried to describe to you is the framework of the condition of shell-shock. There are often as well added symptoms—dilated pupils, pallor, stutters, nervousness of manner—which do not add appreciably to the gravity of the cases. But there are a number of symptoms that do add considerably to the gravity of a case, and make the prospect of the sufferer's doing good work in any reasonable time extremely poor. These symptoms are :—

1. Loss of memory.
2. Constant mental confusion.
3. Hallucinations (uncommon).
4. Epileptiform fits.
5. Petit mal.
6. Profuse sweating.
7. Clinical picture of exophthalmic goître.

I have seen cases which were unquestionably cases of exophthalmic goître, and I had no reason to question

their recent origin. I had two such cases treated with *x*-rays. But neither did at all well, contrary to my experience with these measures in cases of Graves's disease in civilian practice.

Sweating is a common symptom in mild cases of shell-shock. But in mild cases it is neither profuse nor constant. I have seen a man who had been sweating for seven months. At the time I saw him he was drenched in perspiration, and the medical officer in charge told me the man had been sweating continuously and as heavily the whole time. Nothing influenced it or controlled it. I was shown another case that had been sweating unilaterally, and only unilaterally, for many weeks.

It will occur to everyone to ask what is the pathology of shell-shock—what are the conditions underlying its incidence? This question I am unable to answer; my own conception of it is exceedingly vague and crude. Many of its outward expressions—frequent pulse, tremor, dilated pupils, sweating—are classical signs of great horror—and it seems as if the terror that flieth by night and also by day, to which all were exposed, has left greater and more enduring impressions on some nervous systems, especially on some sympathetic nervous systems, than on others. That it is not due to mere mechanical or chemical causes, such as concussion, exposure to noxious or to highly compressed gases, seems certain, for else all would suffer alike. Moreover, I have seen a few cases, which I should have considered typical and genuine cases of shell-shock, in men who had never been nearer the war than the South of England, nor nearer any explosion than that one had been within twenty feet of an accidentally over-turning lorry.

One's general impression of the men who make up this class of patient is that they are below the average in

nervous evolution, and that their brains are of less durable stuff than those of the majority. When one sees several together one is struck by the general dulness in their expressions, and is inclined to size them up as men who in civilian life "follow cattle," or "stand on the step of a bus." I cannot, of course, assert that the men whom I have seen suffering from shell-shock are, when in their normal health, mentally inferior to others. Less still can I assert that they are wanting in courage. On the contrary, I know that many have behaved with very conspicuous bravery. But I think that their reason has less than average control over their primitive instincts, and that it is more easily detached from their general make up than it is in the majority of men. I do not mean that their higher cerebral functions are disordered, so that we in finding them wholly unlike our own must consider the men insane, but I mean that their higher cerebral function—their reason—does not rule their primitive individual life, their sum of likes and dislikes, their emotions so fully as in the majority of men.

It is remarkable how little will cause the return of all symptoms which patient treatment and time have caused to disappear. A petty squabble in the ward, the noisy breaking of a basin, will cause immediately the return of headaches, insomnia, tachycardia, sweating, speechlessness; and it may be long before the symptoms disappear.

And there is this remarkable fact to be noted, that the majority of the men are wholly unwounded. This is so much the rule that it must be taken into consideration in advancing any explanation of shell-shock physical or physiological.

I should like now to relate to you the history of a case of shell-shock which was under my own care and obser-

vation for sixteen months. It is that of an Englishman, twenty-one years of age, in a rifle regiment. In May, 1915, he was sent home, without any history of what had happened him, and was admitted to the Dublin University V. A. D. Hospital in Mountjoy Square. The prominent symptom from which he suffered was dumbness. Besides, he saw very badly, and bumped into things. He was a little deaf too. His pupils were dilated, his skin always moist, and he was restless, shaky, and tremulous and feeble in getting about. His pulse was not remarkably frequent nor its tension raised. He unquestionably saw "things" that did not exist on the wall in one particular corner. He had no exophthalmos that could be regarded as pathological.

He was distrustfully afraid of us all, and we could do very little for a few days but keep him in bed and treat him with all kindness and gentleness. Then I got him up. He had quickly recovered his hearing, and I found he could write. His writing was large, shaky and monosyllabic, but it was the handwriting of one who had formerly written well and even elegantly. He could answer a few questions about himself, but he remembered very little of his home and less still of the war. What little of the war he did remember he kept on writing down—over and over again, catching me by the coat and shaking and sweating (and licking his pencil) he wrote down—"*asylum, do not lock up, I am not mad*"—over and over again this same pleading. Soon we had him reassured on this point; and then I tried to teach him to talk. We had first tried very injudiciously to rush it a little by enploying hypnotism, but as the *séance* began with surrounding his bed by screens he at once felt he was being locked up again, and nothing could be attempted

for several days while his state of perturbation was subsiding. Slowly and painfully he learned letters pp, tt, sss, "a" and "ooo" merely whispers. Then aa-ss, aa-tt, t-oo. Finally after many weeks, ss-sst-r, b-tt-r. He read a little, drew some pictures, not badly, once in a way played the piano, and he occasionally became skittish and made apple-pie beds for the other men. But all the while he was a soft, gentle boy, and one wondered how he had ever survived the drill-sergeant let alone have come to be thought fit for active service. He had a visit from his father at this time, and it is certain that he knew him and could appreciate some of his conversation.

Things went on like this till the end of September, During the last week of that month another man who had been sent home on the same ship also suffering from shell-shock and dumbness, recovered his speech, in another hospital, having been given ether to that end. I decided to give my patient a like treatment; so in his hearing I asked the sister to arrange for it, explaining that I wished to make an examination of his throat that might be painful.

An account of the first case, unfortunately, found its way into the public Press, and I know my patient saw the paper it was in. I do not know that he saw the actual record. So whether he may have been worked on by some powerful suggestive factor of this sort, or whether it was attributable, as many asserted, to his having heard some affecting music that afternoon, I do not know, but I do know that I spent the evening practically sitting on the chest a very different creature from the one I have tried to describe.

He had, it seems been seized about 6 o'clock with some sort of "spasm," shall I call it? and called out by his

nickname to a Highlander in the ward. And thereafter, in a fury of physical and mental excitement, he acted, and revealed to us the only thing known about the origin of his affliction. He was now a comparatively strong man, and he struggled with all his strength. He knew none of us, he had no idea where he really was, he saw only the Germans coming and carrying off his machine gun while he was unable to move. He shouted for men to help, and wrung his hands over their, our, madness in not seeing and coming to the rescue. He seemed quite clear what ought to be done under the circumstances as he saw them; and I understood that he might have been a very good soldier. I had given him almost ½ gr. of morphia to quieten him, and as it began to take effect—it never had very much—his fighting spirit gave place to despair. He felt disgraced by the loss of his gun. He trembled at the thought of what would be said to him, and I had to promise over and over again that I would take the whole responsibility. For three days he continued in this state, only in a less violent mood. He now remembered his regiment, his regimental number, and various things connected with the army and the fighting which he had forgotten before—but his life during the previous months was entirely forgotten. He knew me only as the officer who had promised to say he was not to blame—and all the rest were quite forgotten. One thing only did I discover, showing he had some buried memory of his surroundings. He did not know us, he "had never been in Dublin or Ireland," but when I asked him if he knew B——, his chum, he said "Yes, he had once known a man of that name—he was an Irishman (which he was not), and he lived far away over there," pointing to the ward from which they had both

been moved some days previously. He could not read now, partly "because the print was indistinct," but chiefly because words, when he had spelt them out, conveyed no meaning to him. He had a functional alexia. I tried him with the Bible and a Prayer-book, as I thought he might recognise the words read easily, but he gave up trying, and instead picked up a scribbling book in which he had drawn some pictures. Suddenly, he turned up one—a picture of a bunch of flowers that was still in his locker—and then he had another fit of excitement, and went back to his state of seventy-two hours before, retaining, however, his power of speaking. After this he remembered nothing of his time in France, except that he had been locked up. On that he was definite and positive, but he was reasonably rather than emotionally outraged at the fact. His memory of his home life was more than ever a blank—he had completely forgotten his father, and when visited by him acceped him as his father "only because we said he was his father." He told me quite dispassionately that he found it very hard to keep up a conversation with his father (who was again visiting him), as he knew nothing of his home, and his father knew nothing of anything else.

Naturally he was rather prostrate for some time. However, he soon picked up again, and by the end of October he was physically stronger and more alert than he had yet been. But his mental horizon was bounded by the four sides of Mountjoy Square and what little he could remember of his reading in the newspapers. He had even then some trouble in vision and suffered from headache. And in the beginning of November his sight left him, and his knees failed him, when out in the square, in a manner to make me suspicious of a slight epileptic fit.

It is obvious that there is no special treatment for such a case as this man's. He was kept warm and quiet, and employed as far as possible. I tried various suggestive remedies and various tonic and sedative drugs, but nothing made any striking difference. My colleague, Dr. Smyly, tried hypnotism at the man's request, but it was not a success. As he was worse after it it was discontinued. Psycho-analysis I tried quite ineffectually, as you will surmise, seeing the man remembered nothing in his normal state and hypnotism was barred.

Now at the end of November he became alarmingly depressed, and made several suspicious moves towards the windows. He told me quite frankly that he could look on at himself going out through the window without feeling in the least responsible. His blood-pressure, which had been normal, now registered 178 m.m. Hg. His pulse was 80 to 90 per minute. I asked Major W. R. Dawson, R.A.M.C., to see him with me, and he gave it as his opinion that the man was suffering from melancholia and was suicidal. Under Major Dawson's advice precautions were taken for treating the man where he was. He was kept in bed and given thyroid extract in quantity. He got 15 gr. the first day, 30 gr. the second, 45 gr. on the third, and he would have got 60 gr. on the fourth, 45 gr. on the fifth, and so on had we not become alarmed at the feebleness and frequency of his pulse, which went up to 140 per minute. Mentally he was very much the better of this treatment, and three weeks later his blood-pressure had fallen and was 140 m.m. Hg. But he was weaker and more shaky and more pitiable than ever. He now became frankly hysterical in a ridiculously emotional way and in his inability to do things. He could not stand nor walk, he could not pull on his socks. He

could not even feed himself. I thought a change of scene would do him good, and in February, 1916, I had him transferred to Mercer's Hospital. There he showed no emotional peculiarity at all. He was always nervous and retiring, but not at all unreasonable. He suffered from a functional inability to stand and walk—astasia—abasia—which proved quite incurable. And his tremor became very jerky and coarse and persistent. He also developed gradually during the spring a considerable even enlargement of his thyroid gland, and his pulse was constantly up to 120 per minute. He had well-marked dermatographia, and approached to the clinical picture of Graves's disease. But his chief trouble lay in his inexplicable sleeplessness. Even $\frac{1}{4}$ gr. morphia often had very little effect. Of all drugs, paraldehyde proved the best. At first he had to have a full dose, but by degrees we cut it down to one minim in a spoonful of water, and it acted. But without that he often spent a whole night genuinely and certainly without sleep.

On the whole, there was no improvement, or so little in the spring and summer that I gave the case up as a bad job, and recommended his discharge. He was sent home on the 2nd of September.

A letter from his father at the beginning of this month contains this information about him :—" He has improved considerably during the last two months, and is much steadier. He flushes very much if strangers worry him at all, and is then inclined to be more shaky. He is altogether brighter and more himself. He could never tell us anything about Dublin when he came home, but his memory is much improved, and I don't think he ever forgot who you or sister were. And I am glad to say he can read much better. When he came home first all his

friends were as strangers to him, now he remembers them after they have been a time or two quite well, and takes an interest in them. As regards the walking, he is still unable to support his weight on his legs—still I can see a difference, for he does not take so much supporting now as he did a few months ago. . . . ."

The President said that "family stock" was an important factor in these cases. We must conclude that the shell-shock cases had less stable nervous systems than normal. He recalled the case of a boy who suddenly developed "fits" after some punishment. The nature of these was quite obscure till it transpired that he had recently seen a case of chorea.

Dr. Craig had seen a number of these cases, which he agreed were most miserable. Symptoms developed six weeks or so after some shock—for instance, being partially buried. Sensory phenomena were curiously in common, except retinal hyperæsthesia. Officers frequently exhibited some of the symptoms of exophthalmic goître. With regard to treatment, he had not much faith in suggestion, but had found ionisation often useful.

Colonel Clarke emphasised twitching of the face as an important sign. Sweating was very marked. It should be remembered that many of the cases were miners in civil life, and might therefore exhibit nystagmus due to their previous occupation.

Captain Law frequently found accidental burial the exciting cause. He considered "suggestion" useless, and recommended quiet surroundings.

Sir J. W. Moore commented on the tendency to relapse.

Dr. Crawford thought brain concussion largely accountable. His own experience of shell-fire resembled the effects of a kick on the head.

Dr. Nesbitt asked whether such cases ever occurred in civil life; and, if so, would they not be classed as mental.

Dr. Purser, in reply, agreed that in ordinary cases sensory

symptoms did not occur, but in more severe they did. As regards treatment, the men ought to be employed, but kept out of touch with war matters. He still had some faith in suggestion. He remembered two cases in which civilians were very similar—one developed exophthalmic goître, the other was regarded as insane. He thought some organic brain lesion was probably present, as he had seen somewhat similar symptoms in cases of injury to the skull.

NEPHRITIS AFTER BURNS.

By SIR JOHN MOORE, M.A., M.D., M.Ch., D.P.H. Dubl.; D.Sc. (*Hon. Causâ*), Oxon.; F.R.C.P.I.;

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[Read in the Section of Medicine, May 11, 1917.]

In a very able communication on "Nephritis," read before the Section of Pathology in the Royal Academy of Medicine in Ireland on Friday, January 12, of the present year, my colleague, Dr. William Boxwell, Physician to the Meath Hospital and County Dublin Infirmary, stated that "once in a way it"—that is, nephritis—"follows an extensive burn, though I do not remember having seen such a case."

In the light of this remark by Dr. Boxwell, the following case may prove of interest.

About midday on Friday, March 9, 1917, while going my morning round, I received an urgent telephonic message to visit an elderly member of the medical profession in Dublin, who had been seriously burned. On hurrying to his residence, I found Dr. S. M., aged eighty-seven years, in a state of considerable mental excitement, exclaiming repeatedly that he was not suffering from "shock." He reminded me that he was born on February 1, 1831, so that he had entered on his eighty-eighth year.

It appeared that about 10 a.m. he had got out of bed,

and preparatory to dressing proceeded to warm himself by standing with his back to a brisk fire, clad only in his night-shirt. Incidentally it may be mentioned that the forenoon of the 9th of March was bitterly cold, snow was falling in light showers, borne on a strong easterly wind. There is no doubt that the unfortunate man's night-shirt was drawn towards the grate by the draught, and so set on fire. The doctor was heard shouting in the hall, into which he ran on perceiving what had happened. His maid-servant rushed downstairs to his assistance and was horrified to find him literally in flames. With great presence of mind she rushed into his bedroom—the front parlour—seized a blanket and wrapped him in it in a shorter time than that in which she could afterwards describe what took place. To her presence of mind and promptness the doctor undoubtedly owed his escape from sudden death.

On his own initiative, he had cold wet cloths applied to the burnt surface and he took an opiate.

When I arrived he was fairly easy, so without waiting to strip off the dressing, I hastened to fetch Lieut.-Colonel William Taylor, P.R.C.S.I., having sent for materials to prepare "Carron Oil" as a first-aid dressing. Surgical advice being at hand, I refrained from subjecting an old man to the fatigue of a double examination.

On Mr. Taylor's arrival we found that there were extensive burns of the third degree—for the sensory nerve-endings were evidently involved—implicating the back of the chest, the shoulders, and the right arm. There were minor lesions on the hands and about the occipital region.

As the materials for making linimentum calcis were at hand, Mr. Taylor used this old-fashioned application, in the first instance. We discussed the picric acid lotion

method, but saw no reason to change, especially as Mr. Taylor had knowledge of toxic effects, in connection with delicate kidneys, having followed the application of picric acid. The burnt surface was dressed twice a day—in the evening by the trained nurse in charge of the patient.

On March 10 the body temperature ranged between 101° and 102° F.; the pulse varied from 88 to 96, and the respirations were steady at 24 per minute. On the evening of March 12 the axillary temperature touched 103°. A daily careful examination failed to detect any evidence of pneumonia.

On March 16 Mr. Taylor changed the dressing of the wounds to a pigment recommended by Lieutenant-Colonel A. J. Hull, F.R.C.S., R.A.M.C. It is composed as follows: Beta-naphthol, one-quarter (0.25) per cent.; oil of eucalyptus, 2 per cent.; olive oil, 5 per cent.; soft paraffin, 67.75 per cent., and hard paraffin, 25 per cent. The mode of preparation consisted in mixing the paraffins and olive oil in a jar, then adding the beta-naphthol and eucalyptus oil at a temperature of 55° C. (132° F.). In the cold weather which prevailed at the time it became necessary to melt the ointment, liniment, or pigment, before it could be painted over the burned surface with a camel's hair brush. This application was anodyne as well as antiseptic, nor did it require to be frequently applied.

Although the temperature fell gradually, the patient's general condition did not improve. His bodily powers began to fail, and he complained of increasing weakness. Restless nights were avoided and quiet sleep was secured by a draught containing 20 minims of nepenthe and 10 minims of tincture of digitalis in an ounce of chloroform water.

On March 20 the temperature, which had been coming

down by degrees, rose to 102.5° in the forenoon, and the urine became scanty and was turbid from urates. Remembering that at intervals, in recent years, the urine had been slightly albuminous, I examined it carefully on March 21. It was of normal colour, but somewhat turbid. The reaction was acid. Its specific gravity was 1022. A moderate quantity of albumen was present. On standing it threw down a considerable and heavy deposit. Under the microscope this was resolved into numerous granular tube casts, and vast numbers of spherules of urate of sodium and of colourless uric acid, as well as hexagons of the latter. These rare forms of uric acid are figured in Dr. Hermann Rieder's *Atlas of Urinary Sediments*, [a] Plate XI., Fig. 2, and Plate XXI., Fig. 4 ; and also in a work published many years previously—namely, Dr. Lionel S. Beale's *Kidney Diseases, Urinary Deposits, and Calculous Disorders*,[b] Plate XXVII., Fig. 152, Plate XXVIII., Figs. 155, 156, and Plate XXX., Fig. 166. Going still further back to 1853, we find such uric acid crystals figured in Plate IV. of Dr. Otto Funke's *Atlas der physiologischen Chemie*.[c]

A subsequent analysis of the urine on March 25 gave the following results :—Urine of normal colour, slightly turbid, acid ; spec. grav. 1018 ; moderately albuminous, but free from sugar. Under the microscope several leucocytes, epithelial cells, and many filamentous organisms (probably of extraneous origin) are seen, as well as granular and partly hyaline tube-casts. There are some large

[a] Translated by Frederick Craven Moore, M.Sc., M.D. (Vict.). Edited and annotated by A. Sheridan Delépine, M.B., C.M. (Edin.), B.Sc. London: Charles Griffin & Co. 1899. Large 4to.

[b] Third edition : London : John Churchill & Sons. 1869. Demy 8vo.

[c] Leipzig : Verlag von Wilhelm Engelmann. 1853. Also as a Supplement to C. G. Lehmann's *Lehrbuch der physiologischen Chemie*.

coloured crystals of uric acid, a few colourless spherical crystals of the same, or of sodium urate.

At this date—March 25—the patient's state was very unsatisfactory and unpromising. His mental and bodily powers were ebbing fast, his tongue was coated and dry, and there was complete failure of appetite.

The last phase was ushered in by a violent shivering fit, lasting twenty minutes, in which the patient's bed shook, at 3 30 o'clock on the afternoon of March 26. This was quickly followed by a rise of temperature to 105° F. The pulse ran up to 120 and the respirations to 46 in the minute. Next morning the temperature came down to 102°, the pulse to 108, and the respirations to 40. But the patient was drowsy, and when roused incoherent. The first sound of the heart was failing, faint, and prolonged into a murmur.

The end came at 3 a.m. of March 28, after a nephritis of ten days' duration and on the twentieth day from the accident. Regard being had to the extensive nature of the injuries and to the patient's very advanced age (eighty-seven years), his survival for so long as twenty days bears testimony to his wonderful vitality. It was indeed a tragic ending to a long and busy life of kindly service to his fellowmen. A *post-mortem* examination was not obtainable.

Apart from the interest attaching to the uncommon forms assumed by the free uric acid in the urine in this case, the occurrence of nephritis as a sequel of burning deserves consideration.

In an admirable article on "Burns and Scalds" in the second edition of the *Encyclopædia Medica*,[a] the author,

[a] Volume II., page 544. Under the general editorship of J. W. Ballantyne, M.D., C.M., F.R.C.P.E. Edinburgh and London: W. Green & Son, Ltd. 1915. Royal 8vo.

Dr. Alex. Miles. Senior Lecturer on Clinical Surgery, University of Edinburgh; Surgeon, Royal Infirmary, Edinburgh, states that "if the patient survives a few days, active inflammatory signs may supervene in the intestinal canal, lungs, pleura, kidneys, or meninges, and ulceration may follow in the stomach or duodenum. The relative frequency of these lesions may be gathered from the following statistics of 200 cases by Schjerning :—

"Hyperæmia of brain occurred in	-	49	per cent.
Hyperæmia of lungs	,, -	36.5	,,
Nephritis	,, -	28.2	,,
Pneumonia	,, -	27.0	,,
Hyperæmia of intestines	,, -	22.2	,,
Pleurisy	,, -	18.8	,,
Ulcer of duodenum	,, -	12.4	,,
Meningitis	,, -	10.5	,,""

In discussing the local treatment of recent burns of the first, second, third, and fourth degrees, Dr. Miles holds that "in picric acid we have an agent which meets all our requirements in a way not previously attained by the older methods. It is employed in solutions varying in strength from 1 in 100, which is practically a saturated watery solution, to 1 in 50 when alcohol is added. It may be dissolved in sulphuric ether forming a solution of about 1 in 20, which is painted on to the affected area, and on the evaporation of the ether leaves a covering of a very fine powder of picric acid.

He adds that a useful lotion of moderate strength is made as follows :—

Take of—Picric acid	-	-	1½ drachms.
,, Absolute alcohol	-	-	3 ounces.
,, Distilled water to	-	-	40 ,,

Dissolve.

Dr. Miles states that in a few isolated cases toxic symptoms—nausea, vomiting, diarrhœa, dark-coloured urine, yellowness of skin, and drowsiness—have been alleged to follow its use. "But those"—he adds—"who have had most experience with picric acid have not observed any symptoms attributable to its absorption. The writer, after an experience of over several hundred cases treated by this method, has not yet met with any untoward effects. In young children the solution may be diluted to about half its strength with advantage."

It is to be remembered in connection with this very favourable opinion that Dr. Alex. Miles was one of the pioneers of this method of treatment. His paper "On the Use of Picric Acid as a Primary Dressing for Burns" appeared in the *Scottish Medical and Surgical Journal* in 1897.

The *British Medical Journal*, April 28, 1917 (page 549), reminds us that the paraffin treatment of burns was introduced by Dr. Barthe de Sandfort, of Paris, who described it to the Académie de Médecine rather more than three years ago.[a] Under the heading "Paraffin Mask for Burns," the origin and history of the treatment are detailed in the *British Medical Journal*, July 29, 1916 (page 153). In a letter to the *Times* early last year Mr. Albert Gray, C.B., K.C., Chairman of the French Wounded Emergency Fund, drew attention to Dr. Barthe de Sandfort's method of treating burns by paraffin as practised at the St. Nicholas Hospital, Rue Ernest Renan, Issy-les-Moulineux, Paris.

Dr. Sandfort used a proprietary article to which the name "ambrine" was given. He was led by his experience of the mud baths of Dax, in Gascony, to employ the

[a] *Bulletin de l'Académie de Médecine de Paris*, April 14, 1914.

method in rheumatism and arthritis in 1902. In 1903 he began using it as an application, after cleansing the parts, to ulcers and burns, spraying it on at a temperature of 80° C. (176° F.) and even as high as 100° C. (212° F.), without inconvenience. He considered that one chief advantage of the method was that, after the paraffin had solidified, it exercised a slight but long-lasting pressure upon the part. To increase its toughness and suppleness he added a certain proportion of resins, by preference oil of amber (oleum succini). Hence the term "ambrine."

In his paper at the Académie Dr. Sandfort stated that under the paraffin shell intense lymphocytosis occurs, followed by the appearance of islets of epithelium. These gradually extend, and Dr. Sandfort asserts that cicatrisation finally takes place without contraction.

The writer in the *British Medical Journal* goes on to state that paraffin made fluid by heat has been used for preparing casts of limbs and various other parts of the body, including the head and face.

When Professor Peters, of Toronto, described in that Journal in 1898 a method in which a paraffin spray, followed by an iced-water spray, was employed, the late Mr. Lawson Tait wrote to say that he had used melted paraffin, put on with a rough painter's brush, with the help of a sponge and some iced water in 1864. Dr. E. MacDowel Cosgrave also stated that he had used a similar method devised by Mr. Joseph Lewis, of Dublin.

The paraffin, or "ambrine," treatment of burns introduced by Médecin-majeur Barthe de Sandfort (late of the French Navy) has been adopted by the Naval Medical Service afloat. Sir Arthur May, the Director-General of the Royal Navy, has favoured the *British Medical Journal* with a description of the spraying apparatus employed at present in the Navy. The apparatus is made for the

Service by Messrs. Down Bros., St. Thomas's Street, London, S.E., who supplied the *British Medical Journal* with an explanatory illustration. It will be found at page 549 of the Journal for April 28, 1917.

Another point of interest in connection with the urine, passed in the nephritis stage of this patient's fatal illness, is the uncommon form assumed by the crystallisation of uric acid as regards shape and absence of colour.

Lionel Beale wrote[a] :—"Uric acid deposited in urine can generally be distinguished by its colour from other crystalline deposits, although two or three instances have come under my notice in which the crystals were found to be perfectly colourless."

In Rieder's work we read[b] :—"Under the microscope uric acid appears as yellow, yellowish-green, brown, or brownish-violet crystals, whose fundamental form is the rhombic prism. . . . Uric acid may also occur in the form of four and six-sided colourless plates of varying size, particularly in leukæmia. . . . They differ from the somewhat similar cystin crystals in not being imbricated and in not being [rapidly] soluble in ammonia ; but they are readily dissolved by caustic potash or soda. In addition to these six-sided plates one finds, occasionally, other colourless forms, rounded or oval in shape and having a central depression or radial striation."

The present case does not confirm Dr. Delépine's gloss on the above statement of Dr. Riedel. He adds :—"These forms are probably never found in acid urines, and are the result of unequal solution of typical crystals, when the urine has become ammoniacal." In my patient's case the urine was distinctly acid in reaction, and showed not the least tendency to become ammoniacal.

[a] *Loc. cit.*, p. 371.
[b] *Loc. cit.*, pp. 28 and 30.

PATHOLOGICAL REPORT OF THE ROTUNDA HOSPITAL FOR THE TWO YEARS ENDING OCTOBER 31, 1916.

By ROBERT J. ROWLETTE, M.D., F.R.C.P.I., Pathologist.

[Read in the Section of Pathology, May 14, 1917.]

The stirring events which were taking place in Dublin twelve months ago prevented the presentation to this Section of the usual annual pathological report of the Rotunda Hospital. The present report, therefore, covers a period of two years, from November 1, 1914, to October 31, 1916. In consequence there is a much larger mass of material to be dealt with.

550 specimens were examined, exclusive of 413 specimens of urine sent for special report. Vaccines are still largely used in septic cases on both sides of the house, and their preparation has occupied a good deal of time.

Curettings and other fragments removed for purposes of diagnosis were examined on 222 occasions. One case of tuberculous endometritis was encountered, associated with tuberculosis of the tubes. The great majority of curettings (114 out of 138) showed definite glandular changes, varying from moderate increase in number to considerable hypertrophy, with invagination of epithelium, and in 18 cases formation of cysts. All these abnormalities are, following custom, classed as "endometritis," though it cannot be held that glandular hypertrophy is, by itself, a sign of inflammation. One must have considerable hesitance, indeed, in deciding on the degree of glandular development which is to be regarded as normal.

In the two years, twenty-two cases of malignant disease of the uterus were dealt with by operation—

about our normal rate. Of these cases, 15 were epithelioma of the cervix, and one adeno-carcinoma of the cervix, three adeno-carcinoma of the body, two sarcoma of the body, and one chorion-epithelioma. The proportion of epithelioma is above our average, which for the previous nine years was 56 per cent. of the total.

The case of chorion-epithelioma is the third encountered in my experience at the Rotunda Hospital.

Sixty-three cases of myoma were examined. In twenty-six secondary changes—other than fibrosis which is almost always present—had occurred. The conditions were classified as :—Grey necrosis, 12; red necrosis, 3; fatty, 1; mucoid, 1; œdema, 1; cyst formation, 4; sarcoma, 1; calcification, 1; inflammation, 2.

Of the forty-one cases of salpingitis examined, only 7 were tuberculous. This is much below our previous average of 42 per cent.

Of the forty-eight cysts of the ovary, 4 were dermoids, 8 lutein cysts, 24 smooth-walled cystadenoma, and 13 papillary cysts. Of the latter group—the papillary cysts—more than half—8 out of 13—gave histological evidence of malignancy.

Three solid tumours of the ovary came under examination, of which number 2 were carcinomata, and the other a fibroma.

The number of specimens of lochia examined has further decreased in the last two years, the condition of the hospital as regards sepsis having been satisfactory. It is noticeable that in all the serious cases of puerperal infection the streptococcus was present, either alone or in association with other organisms. It is very rare to meet a case of severe puerperal sepsis due to any other organism than a streptococcus, but such cases sometimes occur, the causal organism being the *Staphylococcus aureus*. The gonococcus was not found in any septic puerperal case in the two years.

APPENDIX.

Table I.—*Examinations of Curettings and other Fragments for Purposes of Diagnosis.*

Endometritis	138	Hydatidiform mole	1
Endocervicitis	5	Chorionic villi, decidua, &c.	21
Erosion of cervix	11	Epithelioma of vulva	2
Epithelioma of cervix	7	Normal, *debris*, &c.	7
Adeno-carcinoma of body of uterus	2		

Table II.—*Varieties of Endometritis.*

Glandular (including cystic)	114	Septic	16
Interstitial	8	Tuberculous	1

Table III.—*Operation Specimens.*

Papilloma of vulva	2	Salpingitis or pyosalpinx (non-tuberculous)	34
Epithelioma of vulva	2	Salpingitis or pyosalpinx (tuberculous)	7
Myoma of bladder	1	Tubal pregnancy	15
Cyst of vagina	1	Double Hydrosalpinx	1
Mucous polypus of uterus	5	Cyst of ovary	48
Epithelioma of cervix	15	Fibroma of ovary	1
Adeno-carcinoma of cervix	1	Carcinoma of ovary	2
Adeno-carcinoma of body of uterus	3	Abscess of ovary	2
Sarcoma of uterus	2	Parovarian cyst	1
Chorion-epithelioma of uterus	1	Cyst of broad ligament	1
Myoma of uterus	63	Sarcoma of large intestine	1
Adeno-myoma of uterus	1	Carcinoma of small intestine	2
Fibroid uterus	7	Inflamed appendix	6
Double uterus	1		

APPENDIX—*continued.*

Table IV.—*Organisms Observed in or Isolated from the Lochia in Forty-two Morbid Cases.*

Streptococci . . .	14	*B. coli*	5
Staphylococci . . .	6	Unrecognised bacilli .	16
Unrecognised cocci . .	19	Negative	7

THE MODIFICATIONS UNDERGONE BY FATTY COMPLEXES IN THE ALIMENTARY MUCOSA.

By F. W. LAMB, M.D.;

Assistant to Professor of Physiology, Trinity College, Dublin.

[Read in the Section of Anatomy and Physiology, May 25, 1917.]

I PROPOSE to examine in outline, firstly, the problems of fat absorption which have been from time to time the subject of investigation, and the methods which have been used for their elucidation; secondly, the histological methods at our disposal; and, lastly, the results which I have obtained from the application of some of these methods together with some generalisations founded thereon.

A review of the lines of experimentation which are reported in the literature shows that a classification of methods and questions at issue may be made on the following lines:—

1. In the first place, *chemical investigations*, which have largely monopolised the field, can obviously give information on the more general points of fat metabolism —the general characters of fatty complexes; the existence and extent of desaturation in the total fat content; various quantitative comparisons between protoplasmic and depôt fat; the distribution of fatty bodies during fat transportation, and problems of like nature.

Yet the application of purely chemical methods has afforded only comparatively meagre results when applied

to fat absorption questions. For example : since Munk (1) showed from examination of the thoracic duct chyle that the intestinal tract was the site of neutral fat synthesis, and Moore and Rookwood (2) approached the site of the process in the intestinal epithelium by an examination of the chyle in the mesenteric lacteals, little was done to investigate the more intimate cellular phenomena until the publication of a paper by Noll (3).

2. On the other hand, it was obvious that it is from *chemical and physical histology* we must expect results when dealing with intracellular processes.

The difficulties and fallacies of such methods are notorious, but the order of the phenomena is microscopical.

The main problems to be solved may be divided into the nature and extent of changes undergone by fatty substances in the intestinal lumen, the method of actual transport into the alimentary mucosa, the phenomena of the deposition of fatty bodies in the epithelial cells, and the relation of the different parts of the cells thereto ; the transport from these cells into the lacteals, the physical and chemical properties of the fatty substances as they exist in the lacteals and in the thoracic duct.

I do not propose to refer in the present paper to several other problems which have been the subject of a certain small amount of work—for example, the changes taking place in lymph glands and the possibility of direct absorption into the blood.

The set of phenomena more immediately concerning me is that presented by the fatty intracellular globules and the staining reactions which can be obtained from the methods at our disposal..

When we review these latter it is convenient to classify

them into—(1) what I propose to call morphological methods, that is, those which afford evidence of the presence of fatty bodies in tissues, but which give slight clues to the individual constituents or to any changes which are taking place; (2) methods which afford some chemical or biochemical information.

(1.) In greater detail the first class, *morphological methods*, may be divided into :—

(*a*) Staining by the use of dyes, "intersoluble" with fatty substances, applied to frozen sections. To this class belong the well-known Sudan III., and Scarlet Red dyes (Daddi, 1896; Michaelis, 1901, *vide* Mann (4)).

The oxazone dye, which is easily formed by intramolecular change from the oxazine dye, Nile Blue (*vide infra* and 5), is another example of this class.

It cannot be too strongly emphasised in this connection that these stainings depend entirely on the question of solubility : the dyes do not stain fatty substances in the solid crystalline condition. It is certainly possible to get different nuances of colour staining with them in a section, but it is impossible to draw any valid conclusion therefrom, as this difference depends on degrees of fluidity or size of the globules (2, a, and Fig. 6). A simple experiment will prove the former. A fatty body, solid at ordinary temperature—*e.g.*, stearic acid—placed in contact with one of these dyes remains unstained until the temperature is raised to the melting point. Similarly, frozen oleic acid does not stain with osmium tetroxide. Pure cholesterol and pure tributyrin do not stain, but when they are mixed blackening is rapid and intense (33).

(*b*) The second general method of procedure is to use some fat fixative, then to imbed the tissue in paraffin. The fixation may have different degrees of action on the

various fatty and lipoid compounds, and thus conclusions may be drawn as to the presence of certain substances which are fixed by the reagent employed, so that they remain undissolved during the embedding procedure, and are made evident by subsequent appropriate staining. The fixative procedures that have been employed are—(1) chromation (*e.g.*, potassium dichromate), and then staining by Sudan III. Bell (6) has employed this method, and has drawn conclusions from the presence of a "Sudanophil" precipitate regarding the occurrence of "lipoid" (*cf.* also Ciaccio 7 and 8). (2) Osmication. Osmic acid, as is well known, renders certain substances insoluble in fat solvents, with a consequent resistance to the solvent action of the paraffin procedure.

This osmication can be direct (9) or followed by alcoholic treatment (10). The latter produces a further reduction where the sole application of osmic acid produces little effect.

(*c*) Observations can be made on the arrangement and form of the vacuolation, in stained paraffin sections. Stewart (11) has used this method in order to observe the effect on tissue elements from the deposition of cholesterol crystals. This procedure has the further merit of allowing observations on tissue changes—*e.g.*, the extent of leucocytic invasion, cell proliferation, or other changes accompanying fat deposition.

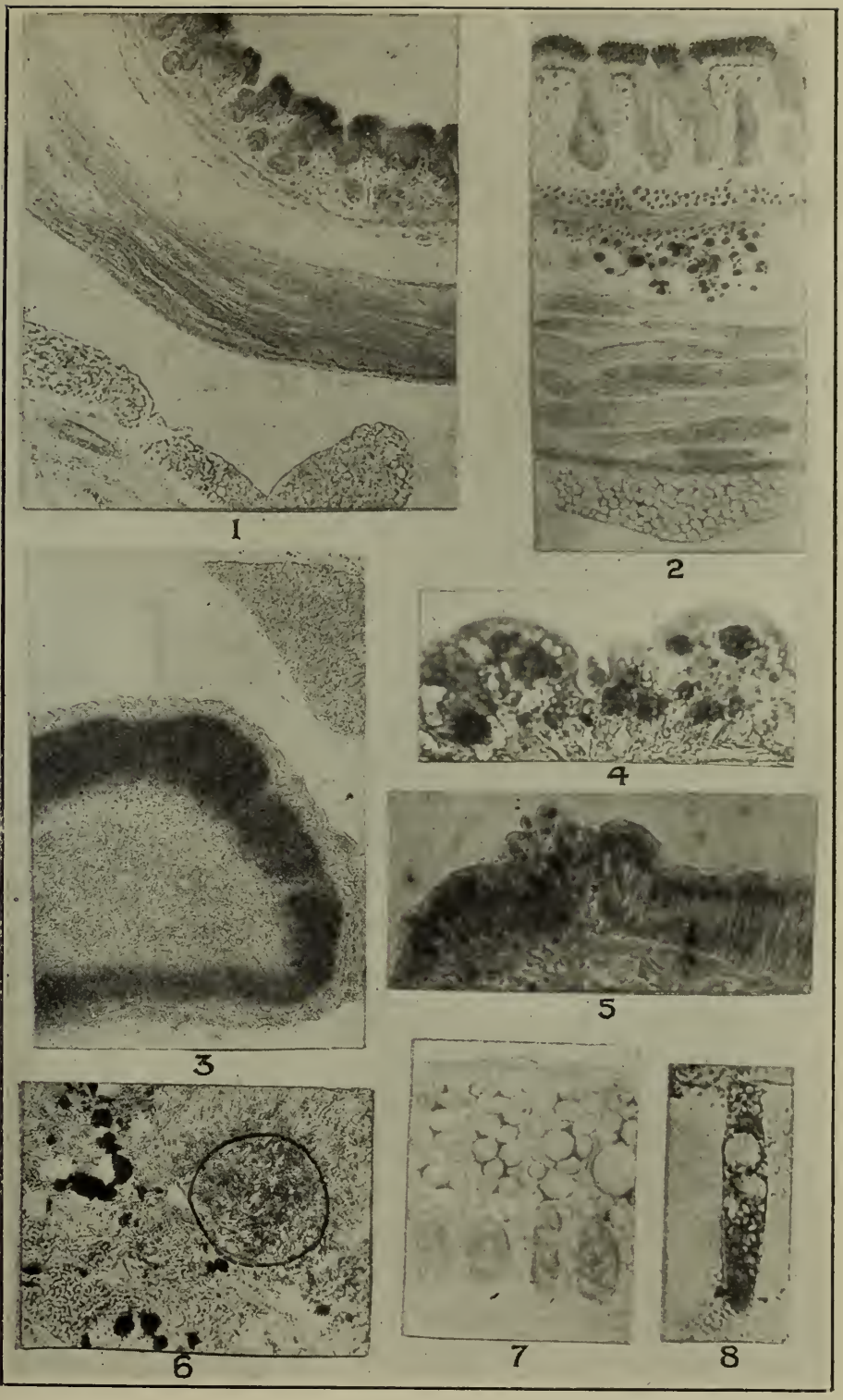
2. Procedures which allow of more or less certain deductions regarding the chemical nature of the fatty complexes. We may refer to them as *chemical and biochemical methods*. It is, however, to be borne in mind that the term chemical is a too narrow one. The physical conditions are of equal or perhaps of greater importance. The transport of fat from the intestinal lumen to epithelial

cell is essentially one of relative solubility (partition) in the intestinal contents and in the epithelial protoplasm. This was overlooked in many of the old discussions on the necessity for soap formation as an antecedent to absorption (24). Further, no fatty globules in tissues are chemically pure—the admixture of minute amounts of lipoids with absorbed fat is of great importance. Again, the question of the conditions which give rise to histological "masking" of tissue fat are probably physical rather than chemical in nature (12).

The methods here referred to may be divided into (*A*) a general group which affords some evidence of chemical properties, and (*B*) a group which, depending for its results on a progressive oxidation procedure, merits special attention.

(*A*) Taking the more general group first, let us see what chemical properties the fats and lipoids present which may form the basis of methodical investigation.

(*a*) "*Unsaturation.*"—The methods available are osmic acid (Altman, 1894) (9) and chromation, with subsequent hæmatoxylin staining; that is the production of a chrome hæmatoxylin lake fixated to the unsaturated linkage (Lorrain Smith and Mair, 1908) (18). As all fatty complexes in the tissue contain unsaturated members, the mere observation of unsaturation of itself is not of great interest, except in the case of the absorption of a saturated fat (*vide infra*). No valid deductions of the amount of unsaturation can be drawn from the degree of osmication, for osmic acid readily yields up its oxygen. This liability in fact limits to a certain extent its usefulness. Further, the physical conditions are of importance. For example, in Fig. 6, the region enclosed in a circle contains a deposit of lipoid globules, which contain many un-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saturated groups in fluid crystalline condition, which are only slightly browned by osmic acid, while in the same neighbourhood can be seen fat cells, whose contents take on a deep black colour. The conditions are probably similar to those which produce difference of colour with intersoluble dyes.

(*b*) *Neutrality.*—The Nile blue sulphate method of Lorrain Smith (5) affords a histological method, which, subject to a certain reservation, allows of observation on this point. The reservation is, that while the red colouring matter (an oxazone) belongs to the group of intersoluble dyes, therefore rapidly staining all liquid fatty globules, whether neutral or with free acidic members, the formation of a coloured soap by the Nile blue base (an oxazine) is a slightly slower process, therefore it is necessary to wait for this to take place before drawing conclusions, and at the same time to guard against the hydrolytic effect of CO_2 on the neutral fats (*cf.* p. 259, section A, *infra*).

(*c*) *Presence of Soaps.*—It has been claimed that the use of Fleming's solution will detect the presence of soaps, but I have never been able to convince myself of their occurrence in normal tissues nor of the reliability of the methods proposed.

The histochemical methods for detecting the presence of soaps require more extended examination before many statements in the literature can be received.

The cuticular zone of the epithelium is a place where, if anywhere under normal conditions, the presence of soaps might be expected, yet I have never been able to detect here any staining phenomena which might point to their presence. The fat here is "masked" in a most remarkable way.

(*d*) *Facility with which the globules undergo hydro-*

lysis.—The method (13) consists in staining with a basic aniline dye and exposing the sections placed in a layer of gum solution to the action of carbon dioxide of weak concentration. The rapidity with which a coloured soap form indicates the facility with which neutral fat globules undergo hydrolysis. It is obvious that this method requires the strictest controls by testing the sample of fat fed under similar conditions and drawing conclusions (from the examination of sections recently fixed in neutral formaldehyde) where a marked difference is apparent.

(*e*) *Solubility in fat solvents.*—In chemical investigations the relative action of the various fat solvents has been widely utilised, and to some extent a similar procedure has been applied in histology. Noll (3), making use of this method, has come to conclusions on glyceride synthesis similar to those which I have formed by the employment of the chrome hæmatoxylin process (*vide infra*).

The staining procedure may be :—(1) Osmication, with subsequent application of the fat solvent; (2) or the staining with Sudan III. or similar dye, of sections which have previously been treated with a given solvent.

The process can be modified by using a fat solvent which has been previously saturated with a given fat or lipoid, but its application is limited in view of the phenomena of intersolubility exhibited by the bodies under discussion.

(*f*) *Polarisation phenomena.*—In the case of lipoidal mixtures which form anisotropic globules between certain ranges of temperature, the polarisation microscope can be used to determine two points—viz., if anisotropic globules are laid down in the cells, and, if present, whether they present the same " clearing point " as that of the mixture given in the food (14).

I may remark that I have seldom met with anisotropic

intracellular globules in the alimentary mucosa, especially of reptilia. Two conditions may hinder their detection: the melting point of these bodies may impede their absorption, and the minute quantities in which they seem to be present may be insufficient to give anisotropism. I have had more success with the method in the case of warm-blooded animals. The phenomena of anisotropism, of course, is not due solely to the presence of cholesteryl-esters (14).

(*g*) *Staining by acid dyes* —viz., the methods of Weigert or Altman with acid fuchsin (15). The lipoids for which this method is of use are lecithin and sphingosin, which have basic groups. Altman's granules are considered by most recent workers to have lipoidal constituents of a phosphatide nature (16), (21).

(*B*) The methods which depend on the "*progressive oxidation*" of unsaturated linkages present very many points of interest, and have opened up many fresh paths for the elucidation of intracellular fat metabolism.

The process consists in the action of dichromate of potassium on fatty acids, fats, or lipoids, which have in their molecule one or more unsaturated linkage. At an intermediate stage the chromium compound is fixed to these links, and so serves to anchor the hæmatoxylin, so that the chrome-hæmatoxylin-fat or lipoid complex resists the bleaching action of borax ferricyanide solution or of the Pal method. That the method only stains unsaturated groups cannot be too strongly emphasised in view of some erroneous statements in the literature. For example, Kawamura in his Monograph (17) has the following statements :—

"Gegen die Smithsche Methode verhält sich das Olivenöl negativ . . . Die reine Stearinsäure färbt sich bereits nach zwei Tagen Schwarz."

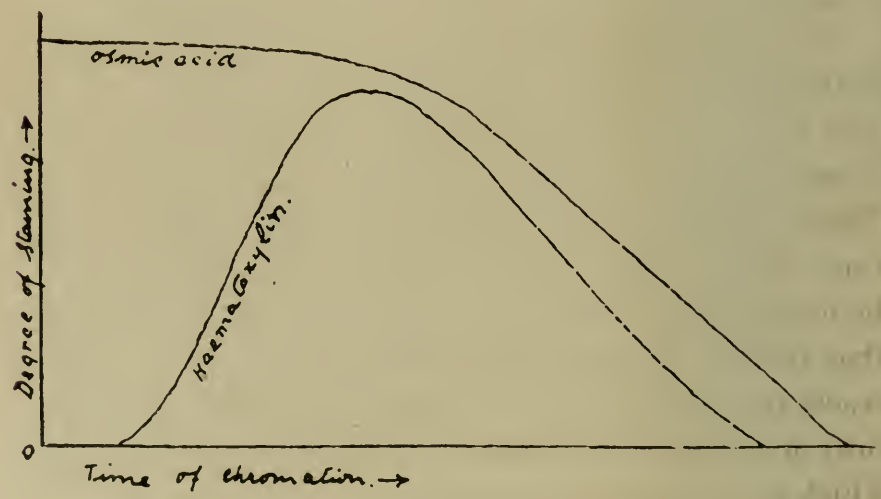
Stearic acid, being a saturated acid, never stains with this procedure, while olive oil stains at the same rate as oleic acid.

The details and theory of the method have been so fully described by Lorrain Smith and Mair (15), (18), (19) that it is unnecessary to discuss them in this place. It may be useful to emphasise how the method may be standardised and comparative results obtained (20). Frozen sections after formaldehyde fixation are placed in a large bulk of potassium dichromate solution of a given strength, and maintained at a suitable temperature.

Artificial sections—*i.e.*, the sample of fatty substances —which has been fed, smeared on pieces of cigarette paper, are treated under exactly similar conditions. At 24 or 48 hour intervals, sections from both series are stained and subsequently bleached for fixed times, and the degree of staining observed. The stains employed subsequent to chromation may be—(*a*) hæmatoxylin, or (*b*) osmic acid.

The following diagram represents the course of the process :—

(*a*) It will be observed that, using hæmatoxylin, there



are three stages: first, unstainable stage, when no chromium complex has yet been formed; second, a stage of gradually increasing depth of staining—this is the stage which I have found most useful; third, a stage of gradual

decreasing depth of staining, followed by the final result, complete saturation (*e.g.*, oleic acid to dioxystearic acid); hence the disappearance of the chromium complex, with consequent inability of the fatty globules to stain.

(*b*) Where osmic acid is employed, the staining is obtained from the beginning, but gradually diminishes in intensity parallel to the disappearance of the double bonds.

The figures (1) (3) show two examples of comparative results which I have obtained, the comparison being in each case with the depôt fat in pieces of adipose tissue in the same sections. Comparison can also be obtained within the boundaries of the same cell (*cf.* Figs. 7 and 8).

I now desire to recount the results which I have obtained by use of certain of the above methods. The animals used have been both warm and cold-blooded. There is, however, considerable difficulty in feeding warm-blooded animals on fatty substances, as it is generally impossible to give these alone, and the presence of other food complicates the results.

As the principal object was to obtain comparisons between the staining reactions of the fatty mixtures fed and those of the resulting intracellular globules, it was found that frogs were the most suitable for experiment. These animals can be used in the fasting condition. Further, by the use of liquid paraffin the alimentary tract can be emptied of its contents. It need hardly be mentioned that before valid conclusions can be drawn, the staining reactions in sections from fasting animals must be carefully investigated, and when specimens from animals in which absorption has taken place are examined, the extent and topography of the absorbed bodies must be ascertained by Sudan III. or Scarlet Red staining.

1. *Neutrality.*—In all cases (with the possible exception

of the gastric mucosa) when free fatty acids or lipoids with acidic properties have been given, the resulting intracellular globules have shown a red stain with Nile blue. In the case of fatty acids, as detailed below, the production of a triglyceride does not occur at once, yet the globules do not take a blue stain : there is no explanation of this anomaly.

2. *Fats or fatty acids of the saturated series* become capable of staining with osmic acid or with the chrome hæmatoxylin method after absorption (Fig. 12).

The staining is not as intense as in the case of unsaturated fats, yet it is quite distinct. It seems to be situated principally at the surface of the globules.

The explanation of this result may be—(1) that the absorbed substances are associated with unsaturated lipoids, present in the cell or derived with them from the intestinal contents, or (2) that the absorbed fats are themselves desaturated.

3. *Progressive oxidation methods* applied to samples fed show different rates of staining before and after absorption. Triolein is the only case where this difference is slight. The acceleration of staining after this fat has been absorbed is one to two days at room temperature with saturated dichromate.

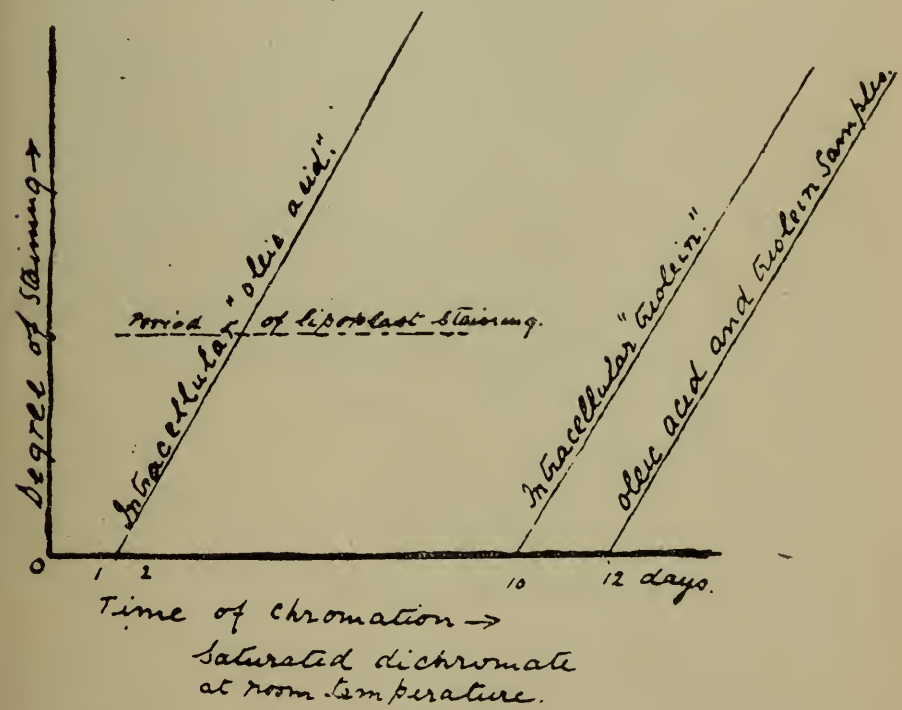
The most remarkable variation is in the case of the free fatty acids. Here the difference amounts to ten days. For example, absorbed oleic acid reaches the stainable stage when treated with saturated dichromate at room temperature in 24 to 48 hours, while free oleic acid and triolein treated outside the body cannot be stained before 12 days. They are stainable at the same rate.

Put in diagrammatic form, the phenomena can be exhibited as follows, p. 265:—In other words, the absorbed

acids are not immediately synthesised to the corresponding glycerides, but there is some intermediate stage in which the globules are easily oxidised. A mixture of free oleic acid and glycerol leads to the deposition of globules similar in behaviour to those appearing when triolein is fed.

Noll (3) came to the conclusion that such an intermediate stage existed. The methods that he used did not afford him an opportunity of observing the acceleration effect. The difference in staining rate can be observed to persist in fat that has passed into the lacteals.

A further detail can be made out. In the case of triolein or oleic acid fed with free glycerol, there is a stage obtainable when the intracellular globules are not yet stainable, but situated between them there are bodies stainable at a very early stage. They are best obtained by the action of 1 per cent. dichromate at 37° C.



This is indicated in the diagram. The appearance of these bodies, which may be named "lipoplasts," can be

seen in Figures 7 and 8 (32). Thus, a differentiation may be made within the limits of one cell as to its fatty constituents (21) (33).

In the case of oleic acid absorption, the stainable stage is reached so soon that it is impossible to observe whether such bodies are present separated from fatty globules.

It might be suggested that they are fused with the globules, and to this fusion is due the early staining, while, when synthesis is complete, these lipoplasts are segregated from the triglyceride, and the latter is now practically depôt fat. It is interesting to note that the lipoids of the adrenal cortex are easily stained. The function put forward for this part of the adrenal is lipoid elaboration (25). What body plays the chief *rôle* in these phenomena? Elsewhere I have suggested cholesterol (20), (31).

The evidence for this view is—firstly, mixtures containing cholesterol are easily stained; secondly, cholesterol is able to antagonise the irritating effects of free fatty acids (*cf.* Fig. 10). I have reported elsewhere this action of cholesterol (22) (*cf.* 23, 25).

As to the source of the cholesterol, it is known as a universal constituent of protoplasm, so that it may be present in the cell, or it may come from the bile. The latter source is supported by the observation that oleic acid in the intestinal contents is already easily stainable.

4. *Anisotropic lipoids* and lipoidal mixture are rarely detectable as such after absorption. I have already referred to this. It is further evidence of the modifying influence in the intestinal mucosa.

5. The morphological pictures are different, both as regards the *type of vacuolation*. This can be easily seen in paraffin section (*vide* Figs. 9, 10, and 11).

This difference can also be seen in frozen section treated

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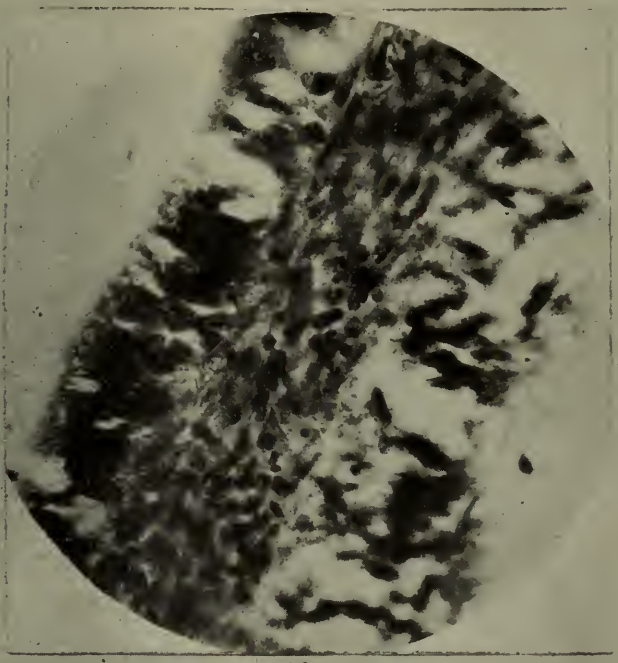

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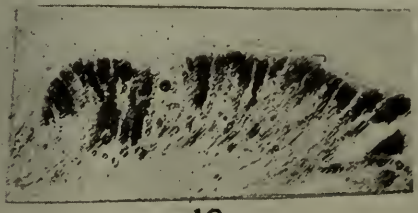

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by the hæmatoxylin method (*vide* Figs. 4 and 5). Other points can be made out from inspection of the intestines, where fat absorption is going on, and from paraffin sections. As already noted, free fatty acids have an irritating effect, and may even cause the death of an animal. The intestines show injection of the capillaries and epithelial desquamation (Fig. 10). This is an extreme stage. With the feeding of other mixtures varying degrees of vascular reaction can be seen, both as regards the extent of leucocytic emigration (Fig. 11) (varying chemotactic influence?) and as regards the degree of the vascular dilatation.

6. *Fat absorption in the stomach* presents several points of interest, to which I drew attention some years ago (20).

Subsequently other observers investigated this site of absorption (26), (27), (28).

The figures 1 and 2 show the histological picture that can be obtained. The special points which I have observed are—(*a*) the fat , both intra-epithelial and in the submucosa, is as easily stainable by the chrome hæmatoxylin method as that in the adrenal cortex. It is, therefore, quickly oxidised.

In the case of the fasting frog there is a continuous row of globules staining with fat stains below the free border of the epithelial cells on the surface. These may modify the reactions of absorbed fat, but in the case of warm-blooded animals I have not been able to observe this appearance.

This facility of staining cannot be due to cholesterol from the bile, as these phenomena can best be observed in the region of the cardia. It may be due to the absorption by the stomach of fats which are easily oxidised by the method or to a mixture with cholesterol which has been supplied

by the cells. (*b*) The staining with basic aniline dyes is more rapid than the staining of fat in the intestinal mucosa from the same animal which has been fed on a mixture of fat, such as occurs in milk. In other words, it is more easily hydrolysed under like conditions. Nile blue stains the globules and neutral tint.

(*c*) In the same animal the amount of fat in the gastric mucosa is in inverse proportion to that in the intestinal mucosa. Obviously, the stages of absorption in the stomach precede these in the intestines.

It is hard to conceive that soap formation can take place. I am inclined to the view that the epithelial protoplasm has a solvent affinity for at least certain fats or fatty acids. This is supported by cases where I have found some evidence of fat absorption in the frog's œsophagus. It is also in harmony with the slight absorption which is stated to take place in the intestines after ligation of the bile and pancreatic ducts.

7. *The influence of the nucleus of the cell* on fat deposition (29). It is difficult to decide if such an influence exists. I have noticed in the case of lower members of the fatty series that the side of the cell next the intestinal lumen tends to be free from globules, which increase in number in the nuclear region. In Fig. 12 it can be seen as a fairly broad zone, free from fat.

8. *The importance of triolein or oleic acid.* I have been often struck with the difficulty of obtaining absorption with fat mixtures which do not contain triolein or oleic acid. The glycerides of the lower saturated fats, though liquid at ordinary temperature, are not absorbed easily.

I cannot say whether saturation of the carbon chains has anything to do with this. It may be that triolein and oleic acid are especially soluble in protoplasm or fatty acids of the unsaturated series are easiest of absorption.

9. I have endeavoured to find if the bodies which I have named lipoplasts have a relation to *Altman's granules*, but hitherto I have failed. In fact, so far as my observations go, it would seem that the Altman bodies disappear where fat absorption is taking place.

The main conclusion which I would draw is that the *alimentary epithelium is the primary site of fat specialisation*. Although fat foreign to the animal's body can be laid down in fat depôts, yet the vital activities of the cell endeavour either to absorb fatty mixtures which resemble the fat peculiar to the species or else to produce transformations which lead to this specialisation (30).

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EXPLANATION OF PLATES.

1. Stomach, kitten : frozen section. Saturated potassium dichromate at room temperature for one day : x 80. Depôt fat in mesentery unstained.

2. Stomach, cat : frozen section. Saturated dichromate at room temperature for one day : x 60. From a drawing. Fat in epithelium and in submucosa stained. Depôt fat unstained.

3. Adrenal, cat : frozen section. Saturated dichromate at room temperature for one day : x 80. " Fat " in cortex stained. Depôt fat unstained.

4. Intestine, frog : oleic acid and cholesterol feeding. Saturated dichromate at room temperature for three days : x 330.

5. Intestine, frog : oleic acid feeding : frozen section. Saturated dichromate at room temperature for three days : x 330.

6. Wall of gall-bladder (cholelithiasis). Formol fixation. Frozen section. Osmic acid. x 300. The cells within the circle are loaded with anisotropic lipoids but stain imperfectly. The larger blackened droplets are isotropic.

7. Intestine, frog : triolein feeding : frozen section. One per cent. dichromate at 37 °C. for two days. x 450. From a drawing.

8. Intestine, frog : triolein feeding : an epithelial cell. x 450. One per cent dichromate at 37° C. for three days.

9. Intestine, frog : triolein feeding : paraffin section : x 335. Hæmatoxylin and eosin. Shows type of vacuolation.

10. Intestine, frog : oleic acid feeding : paraffin section : x 335. Hæmatoxylin and eosin. Shows desquamation of epithelium.

11. Intestine, frog : oleic acid and cholesterol feeding : paraffin section : x 450. Hæmatoxylin and eosin. Shows type of vacuolation and leucocytic infiltration.

12. Intestine, frog : Tributyrin feeding. Frozen section : x 80. Saturated dichromate at room temperature for four days.

ABSTRACTS OF SHORT COMMUNICATIONS AND EXHIBITS.

Section of Surgery.

Friday, October 27, 1916.

Screening and Radium Dosage.

Captain Walter C. Stevenson read a paper with this title. He uses the total amount of ionisation effected on the tissue by the radio-active agent as his basis of measurement. The total amount of ionisation is obtained by multiplying the average amount of radium emanation used in a treatment by the time in hours it is employed, and by the proportion of activity remaining after the rays traverse the screen, as shown by the electroscope. He cited his experience with a patient having an extensive port-wine mark where a similar extent of superficial reaction, as far as could be judged by the naked eye, resulted from radiation without a screen, and with various thicknesses of lead screens, provided the loss of activity of the emanation due to screening was compensated for by longer exposure, so that the total ionisation was the same in each case. He pointed out the importance of the screening effect of the tissues on the uniformity of radiation, basing his contentions on electroscopic experiments with various thicknesses of beef, from 1 millimetre to 30 centimetre thickness. He found that lead had approximately ten times the screening power of animal tissues. As an illustration of an efficient dose, he alluded to a patient shown to the meeting, who, to all appearances, was cured for over a year of a tumour of the palate, 2 centimetres across, which was proved histologically to be a very malignant type of epithelioma. The patient was treated with six radium emanation needles for three hours, the dose being recorded as 23.3 (6) 3 = 70 millicurie hours. He pointed out that in the estimation of a dose, the cubic contents and shape of a tumour, the amount of screening

and the number of foci of radiation used in a given area must *inter alia* be taken into consideration. Employing needles of a standard thickness of steel, the amount of screening from them is constant, while the loss of activity due to the tissues —*i.e.*, the uniformity of radiation—depends on the distance of the needles from one another.

Section of Obstetrics.

Friday, November 10, 1916.

Endothelioma of Ovary.

The President of the Academy (Dr. Purefoy) showed a specimen of the above, with the following history. The patient from whom it was removed had sought the advice of Dr. Ashe, because, though only 36 years of age, menstruation had been absent for nearly a year without any evidence of pregnancy or of any manifest disturbance of general health. Her first and only child was born seven years ago. Pelvic examination revealed no evidence of uterine disease. The right ovary was enlarged to the size of a walnut, though not very sensitive to touch; the left was manifestly smaller than normal. Though there was no complaint of pain, the removal of the right ovary was advised, and easily effected, followed by an easy convalescence. Its appearance, except that its surface was very pale, did not arouse suspicion, but under the microscope the characteristics of endothelioma are plain. The President expressed the hope that in discussing the specimen those present would favour him with their views on the wisdom, or otherwise, of removing both ovaries; and, secondly, whether extension or recurrence was likely to occur.

Dr. Rowlette said that on cutting the ovary across, more than half its substance was found to be a soft, homogeneous, friable tumour, which approached closely to the peritoneal capsule, but did not appear to infiltrate it. Microscopically, this tissue was seen to be composed of large, flat, irregular, endothelial cells; in many places they were grouped around open lymph-clefts. The tumour is certainly irregular, and is to be classed as an endothelioma.

Sir William Smyly said they must remember that having heard that the tumour of the ovary was an endothelioma, the case presented itself to them in a different light from what it appeared to the operator, who did not suspect that it was malignant. And under such circumstances, and considering the age of the patient, he thought he was fully justified in leaving the second ovary.

Uterus removed because of Obstetrical Rupture.

The President of the Academy also showed the body of the uterus, removed by abdominal hysterectomy from a parturient woman shortly after rupture had occurred. She was a young 3-para, having a history of difficulty in former labours owing to slight pelvic narrowing. Soon after admission a brow presentation was diagnosticated. This was converted into a vortex, and the presenting part had nearly reached the outlet when signs and symptoms of ruptured uterus became manifest. On making abdominal section, the fœtal body and extremities came into view, having escaped through a large, irregular rent involving the anterior vaginal fornix and lower uterine segment. In the presence of such conditions the immediate question to decide is, after a rapid extraction of the fœtus, whether to repair or to remove the uterus. The former plan he regards as a counsel of perfection, to be kept steadily in view, and to be carried out if the patient's condition warrants the attempt. In the present instance, removal seemed the wiser course, and it was concluded with all possible speed. Convalescence has been slow, but seems likely to be quite satisfactory.

Dr. Hastings Tweedy said that he had, through the kindness of Dr. Purefoy, an opportunity of seeing his patient and helping at the operation. Tears such as this one, he believed, were always due to some previous tearing of the cervix, and the commonest cause of such a tear was the application of forceps before the os was fully open. When once the cervix was torn it was an easy matter to have an extension of the rent, though an interval of years might elapse between the primary and secondary ruptures. In respect to

treatment, he believed that better results all round are obtained by means of a plug than through abdominal section. The plug had two objects in view:—(1) To stop immediate bleeding; and (2) to counteract shock by preventing prolapse of the intestinal contents and the entrance of air or micro-organisms through the vagina. Very few people understood how to apply a plug. At one autopsy he had observed a ribbon of iodoform gauze pushed up to the surface of the liver. This, he need hardly say, did more harm than good. A large piece of gauze should be bunched in the hand, as a housemaid bunches a duster, and this large mass should be pushed into the rent, but not through it. In this way hæmorrhage is controlled, and the intestinal contents are prevented from prolapsing. It is a misnomer to talk about immediate abdominal section for rupture of the uterus. In the majority of instances it takes over an hour from the time operation is decided upon to its actual accomplishment.

Sir William Smyly said that the cause of the rupture in this case was obscure. But in a large proportion of cases of malpresentation of the head it was held in its faulty attitude by the lower uterine segment, and for that reason attempts at rectification were necessarily attended with risk.

Section of Pathology.

Friday, November 17, 1916.

A Primary Pulmonary Tumour.

Dr. Pollock showed the *post-mortem* specimens and microscopic preparations from this case. The patient was a woman, aged about 37, who developed a pain in the left side, together with a dry cough, practically devoid of sputum. Previous to her admission a correct diagnosis had been arrived at. An *x*-ray plate then showed an extensive shadow, occupying almost the entire left pulmonary area, together with a shadow corresponding to the upper right lobe. The pain increased in severity, and laryngeal stridor

set in in addition. Soon after admission the patient succumbed. *Post-mortem* findings showed a tumour of peculiar pearly lustre and soft consistency supplanting the left lung —except for a small basal portion—and tending to envelop the heart. At no point was this tumour adherent, the entire mass being freely movable, like a normal lung, with the exception of the pulmonary apex. At this point it was necessary to tear through the tissue before removal was possible. A similar, but smaller, mass occupied the right upper lobe. The superior mediastinum was entirely free, and no connection appeared to exist between the left and right neoplasms. The sections showed a fairly constant grouping of many layers of large and small polygonal cells around spaces, suggesting lymph spaces or blood capillaries. Such an arrangement might be considered to correspond to the type of endothelial tumour, known as perithelioma. The presence of necrosis in these areas farthest from these spaces would support a belief that they were concerned in the nutrition of the tumour cells.

A Uterine Tumour for Diagnosis.

Dr. Pollock also showed a uterus from a patient of about 60 years of age, who suffered for about nine months from hæmorrhage and a rapidly-growing abdominal tumour, which proved at operation to be an enlarged infiltrated uterus. No further details were provided with the specimen when sent to him. The tumour was of a sarcomatous consistency, composed of softish, buff-coloured masses. Microscopically, the most difficult and characteristic areas that could be found showed parallel channels, enclosed by cells of a somewhat spindle type, appearing to line these spaces, and based upon a definite stroma. Numerous blood vessels, together with hæmorrhage, were also present. Other areas, again, contained small circular cells, with affinity for basic stain, arranged in loculi, and comparable to a carcinoma. He offered the specimen for a histological diagnosis.

The Section leaned to a diagnosis of an endothelial type of tumour.

Section of Obstetrics.

Friday, December 8, 1916.

Dr. Alfred Smith showed—(1) a uterus, with appendages, which he had removed from a patient aged 27, the mother of six full-term children, the last three months ago. She consulted him for hæmorrhage, which was continuous for the past fourteen days. Odour fœtid. A large quantity of *débris* was curetted out of the uterus. The opinion that the uterus was malignant justified immediate operation. The patient said her last labour was normal in every way; no hæmorrhage.

Dr. Smith also showed a large mass of adeno-carcinomatous tissue which he had removed from the peritoneal surface of the recti muscles; also two large nodules removed from the omentum. The patient from which the tumours were removed was aged 43. She consulted him for hæmorrhage without fœtor. Examination under ether gave the impression that the tumour felt was an ordinary fibro-myoma of the uterus. Operation revealed no connection whatever with the uterus. The uterus and ovaries appeared normal to view. The patient looks and feels well.

Dr. T. T. O'Farrell gave the following account of the specimens:—

1. *Tuberculosis of the Uterus and Appendages.*—Naked eye appearance: Several miliary tubercles were present upon the peritoneal covering of the uterus, also on the left broad ligament adjacent to the ovary. Both tubes were slightly enlarged, and contained caseous material, the tubercular condition being more marked in the left tube. Both ovaries were slightly cystic; the larger, the left one, being the size of a large walnut.

The uterine mucous membrane was somewhat ragged and hæmorrhagic, but showed no naked-eye evidence of tubercle.

Microscopical sections taken through the entire substance of the uterine wall showed typical giant-cell systems beneath

the peritoneum, in the muscle wall, and between the tubules of the uterine mucous membrane.

Giant-cell systems and caseation were also found in sections through the left Fallopian tube.

2. *Tumours from the Abdominal Wall and Omentum.*—Three tumours presented the following characters:—

(*a*) the largest, measuring $12 \times 8 \times 4$ cm., was a somewhat flattened, irregular, nodulated and solid mass, without any definite capsule. One flattened surface was covered by peritoneum, to the edges of which were attached some shreds of omental fat. The cut surface of the tumour presented the appearance of a cirrhous cancer.

(*b*) The second tumour mass measured $9 \times 5 \times 35$ cm., and showed much the same appearance as the first specimen.

(*c*) The third and smallest neoplasm was about the size of a walnut, and was embedded in omental fat.

Microscopically the tumours were similar—viz., they consisted of acini lined by cubical or flattened columnar epithelium embedded in a fibrous tissue resembling a fibro-adenoma of the breast. In places, however, the epithelium assumed a distinctly malignant character of adeno-carcinoma. The malignant appearance was most marked in the small tumour from the omentum, where an encephaloid type of carcinoma was present.

He (Dr. O'Farrell) said he regarded the tumours as being secondary deposits from the ovary, uterus, or possibly the intestine, though there was no clinical evidence of the existence of any primary tumour in these organs.

Sir William Smyly said he thought it would be more correct to say that tuberculosis of the uterus was rarely diagnosticated than that it was of infrequent occurrence. Dr. Jellett and Dr. Rowlette had made investigations in this direction, and he thought the result of their work proved that it was a disease of comparative frequency. Dr. Smith's specimen showed more serious pathological changes than were often met with, and justified its radical treatment; but, as a rule, he (Sir W. Smyly) did not think it necessary or advisable to remove a uterus because it was tubercular. It

was invariably a secondary focus, and unlike the same disease in the Fallopian tubes, did not tend to suppurate or caseate.

Dr. Solomons said that if all the uteri were removed from patients who had had the tubes resected for tubercular salpingitis, he was sure a great number would be found to be tubercular. The case reported was very evidently tubercular endometritis, secondary to tubal disease. He thought it most likely that the tubercular lesion had been present for many years in a mild degree, and that the last pregnancy had caused an exacerbation of the disease.

The President said it was particularly interesting to find a uterus so extensively tubercular in a woman who had had six pregnancies, and the last of which was only three and a half months previous to operation. The balance of opinion appeared to be against the removal of the uterus for tubercular disease, but when the tubes were extensively diseased and had to be removed on account of symptoms due to effects of the tubal trouble, he thought, if the uterus also showed tubercular lesions, it would be a reasonable precaution to remove it and extirpate an obvious lesion.

Section of Pathology.

Friday, February 9, 1917.

Hypernephroma.

Dr. H. T. Bewley and Dr. J. T. Wigham showed a specimen of the above condition. The patient, a man aged thirty-five years, and quite healthy, began to complain of a full feeling in his stomach in July, 1916. His bowels became confined and his appetite became poor. In August or September he passed blood in his urine. Early in November his abdomen began to swell, and dropsy of the feet appeared. He came into the Adelaide Hospital at the end of the month. On cystoscopic examination the orifice of the right ureter was seen to be inflamed: streptococci were found in the urine;

otherwise the urine was healthy throughout his stay in hospital. Mr. Gunn opened his abdomen in December, but found a tumour, which proved impossible to remove, so the wound was closed, and healed normally. The patient gradually became weaker, and died in the middle of January. The main specimen is a matted mass of tissue, which was removed with much difficulty from the abdomen. It is mostly composed of tumour masses, everywhere necrotic, and consisting of semi-liquid autolysed materials. At one point a thin layer of stretched-out kidney tissue can be seen, covering a mass of tumour. For the rest, no other normal structures can be made out. The opposite kidney appears to be larger than normal through compensatory hypertrophy. The tumour mass was adherent to the under surface of the liver, through which it had evidently spread. The liver, accordingly, is large, and mottled with white patches—some few as large as a nut, but for the most part miliary, or in groups of small confluent spots. None of the original tumour that was examined showed anything on microscopic section, being completely necrotic, and only a few tumour cells could be made out in the liver, these also being in the same condition. The cells seen were large, with a clear cytoplasm and round nuclei, with a tendency to be arranged in columns or like ducts.

Prof. McWeeney drew attention to the danger of secondary metastasis during the surgical removal of hypernephromata.

Dr. R. J. Rowlette mentioned a case which had come to his notice of hypernephroma of the right kidney with secondary deposits in the liver. The left kidney of this patient seemed to show distinct nephritis.

Organs from a Case of Addison's Disease.

Dr. H. T. Bewley and Dr. J. T. Wigham also showed these specimens. The subject, a man aged fifty-five years, began to suffer about the beginning of October, 1916, from weakness and loss of appetite. These symptoms steadily increased, until after three months he was quite prostrate. He was admitted to the Adelaide Hospital

with symptoms of very great general weakness, weak heart, very feeble and low-tension pulse, and slight darkening of the skin. This last sign was most marked on the back of his fingers, and the pigmentation formed a marked contrast with the nailbeds, which were quite white. The subcutaneous fat was well developed. The patient died one week after his admission to hospital. The specimens shown are both kidneys, with the adrenals attached. When removed the adrenals presented the same appearance, being fairly tense, rounded, and giving some fluctuation. That which was incised yielded a thick creamy pus. The whole, kidneys and adrenals, were embedded in a very tough, yellow fat, as hard as mutton fat, and most of the fat in the body was of the same consistency. The right lung had a nodule in the adherent apex, which is evidently an old cured tubercular focus. Sections of the nodule in the lung showed fibrous tissue and pigment, but no active tuberculosis. On section of the adrenal remains they were seen to be composed of a fibrous capsule and tubercular granulation tissue, with caseous material inside. No other sign of tuberculosis was found.

Cultures from a Case of Weeping Eczema of the Hand.

Dr. W. M. Crofton showed cultures from a case of this condition. It was of eighteen months' duration, which had resisted all forms of treatment. Two micro-organisms had been isolated: one was a streptococcus, which grew with exceptionally large colonies; and the other a Gram-negative, non-mobile bacillus, producing acid in glucose, slight acid after long incubation in lactose, acid and clot in litmus milk; no change in mannite, dulcite, or saccharose. The streptococcus liquefied gelatine: this micro-organism was not agglutinated by the patient's serum.

Exophthalmic Goître.

Dr. A. R. Parsons demonstrated specimens from a case of this disease. The patient, aged thirty-four years, had been two months in hospital; he had the typical signs and symptoms of the disease. He had been feeling well up to

the day of his death, and till that time his condition had given rise to no anxiety. At mid-day the patient gave a cry, became cyanosed, had marked dyspnœa, and died suddenly. At the *post-mortem* examination the thymus was found to be greatly enlarged.

1. *Chronic Endocarditis.* 2. *Intestinal Tuberculosis.*

Dr. A. R. Parsons also showed: 1. a specimen of chronic endocarditis with marked calcification of the mitral valve, and 2. a very typical specimen of tubercular ulceration of the intestine.

Malignant Endocarditis.

Dr. T. Gillman Moorhead showed a heart illustrating very acute malignant endocarditis. He had seen the patient about two months' previously, suffering from acute pneumonia. Three weeks after the pneumonia an empyema developed, and this was evacuated by a surgical operation. The wound rapidly healed up, and the patient was just about to return to his home, apparently completely well, when his temperature ran up suddenly, and he had a shivering attack. On examination an aortic systolic murmur was audible, so a diagnosis of malignant endocarditis was made. This was confirmed by the presence of albumen in the urine two days later, and by the finding of streptococci in the blood. The heart was interesting: it showed very acute pericarditis. One cusp of the aortic valve was almost completely destroyed, while the other two were healthy. Immediately above the diseased cusp the aorta was perforated, the perforation leading into a sack about the size of a hazel nut.

Chronic Endocarditis.

Dr. W. Boxwell showed a heart with chronic endocarditis, upon which was implanted a subsequent acute process. The patient, a man aged 44 years, had a previous history of endocarditis. He had recently developed acute nephritis, with an apparent recrudescence of the endocarditis. The patient had no fever, but became markedly dropsical,

and died. At the *post-mortem* the heart was enlarged, and exhibited chronic pericarditis. The aortic and mitral valves showed marked evidence of chronic endocarditis, but, in addition, there was recent ulceration with the formation of vegetations. The vegetations on the mitral valves extended up into the auricle. The spleen was enlarged, and contained a large white infarct.

Friday, March 9th, 1917.

A Mesenteric Tumour.

Dr. Pollock demonstrated a specimen of mesenteric tumour together with microscopical preparations. The tumour was removed *post-mortem* from an elderly diabetic, suffering from intestinal obstruction, whose general condition precluded operation. The tumour was situate in the middle line, and was freely movable. Upon opening into the abdomen at the autopsy a large abscess cavity was drained, whose anterior wall was formed by the abdominal muscles, the lateral and posterior limits being composed of tumour mass. At no point could stenosis of the intestine be demonstrated, but a remarkable condition was present—namely, that in at least two places the lumen of the bowel was in continuity with the cavity or pus sac in the centre of the tumour.

Much critical discussion of this statement ensued, but—

Dr. W. D. O'Kelly satisfactorily proved the truth of the observation—that the cavity in the neoplasm formed a large expansion or chamber, with ingress and egress through adjoining loops of small intestine. No metastases could be found. Histologically, the tumour was composed of cells varying in size, polygonal in shape, neither showing strictly carcinomatous arrangement nor appearing to be sarcomatous. The presence of numerous cell inclusions was a very pronounced and constant feature of the section, and some multinucleated masses were present. He ventured a diagnosis of primary malignant neoplasm derived from the cells of the peritoneum.

Psammoma from the Choroid Plexus in the Fourth Ventricle.

Dr. T. T. O'Farrell exhibited this specimen. It had been forwarded to him by the late Dr. Charles J. Stack some time prior to the latter's death. The patient from whom the specimen had been taken *post-mortem* was a man aged 55 years. On admission to hospital he gave a history of a blow on the head some three years previously, which, however, seemed to have bearing on the case. His mental condition was that of melancholia, with hypochondriasis. He was emaciated, and looked older than he stated. The knee jerks were absent; Romberg's sign was present, but Babinski's was absent. There was a slight amount of ankle clonus. The patient had optic atrophy, and for some time before his death had paraplegia. At the *post-mortem* the meninges were rather thickened, and adherent; there was some flattening of the cerebral convolutions; the lateral ventricles were distended, and showed some granulations; the cerebro-spinal fluid was increased in amount. Tucked away in the lateral corners of the fourth ventricle were two small tumours, each about the size of a small pea, but one slightly larger than the other. These tumours were attached to the choroid plexus, and had no connection with the pontine or medullary substance. Microscopically, these tumours consisted of a rather loose, delicate, vascular connective tissue network, in the meshes of which were numerous small concretions. These were laminated, and had a circular outline; some of them exhibited a lumen, in which were red blood corpuscles. It was evident that the calcification was distributed around the small vascular spaces.

Section of Anatomy and Physiology.

Friday, March 16th, 1917.

Transposition of Aorta.

Prof. A. F. Dixon showed a specimen in which the aorta was present on the right side of the body. The vessel arose in

the usual manner from the left ventricle, but then arched over to the right side, and descended on that side through the thorax and abdomen. The ligamentum arteriosum connected the pulmonary artery with the left subclavian artery.

The Food Consumption of Working Class Families in Great Britain and Ireland.

Prof. Thompson calculated the food value of diets of working class families collected by the Board of Trade in 1903-4. The figures are divided into two groups—urban and rural; and further sub-divided into five groups, according to wages. Under 25s. per week, the food value of the diet, calculated on the recognised standard, was hardly sufficient. The figures for the Midlands of England are in all groups generally the lowest, and the Irish figures tend to be highest. The amount of protein taken tends to rise with increasing wages. The amount of energy taken in the form of bread decreases with increasing wages. At 25s. a week and lower, over half the energy is from bread; at 40s. and above, 42 per cent.

The Reaction Regulator Mechanism of the Blood.

Prof. Milroy presented results of an inquiry into the manner in which the blood reaction varied under different CO_2 pressures. With CO_2 increasing pressures within the limits normally met with, the reaction of the blood plasma varied very slightly. This is due more to the proteins of the blood than to the salts. The same is true of the contents of the corpuscles, which, however, show a more acid reaction. In soldiers suffering from "trench heart" the reaction of the plasma altered more rapidly under increasing CO_2 tension. This is apparently due to some alteration in the salt content.

Section of Medicine.

Friday, March 30, 1917.

Sudden Death in Tabes Dorsalis.

Dr. A. R. Parsons read a paper with this title. He related the facts of two cases of tabes dorsalis in which

sudden death occurred. The first patient, a man aged 41, had consulted him two years before, complaining of some difficulty in walking and unsteadiness, noticed chiefly while washing his face. There was also slight trouble in micturition—at first difficulty in passing water; later, some loss of control. Seventeen years previously he had a sore on the glans penis, but no other symptoms. His wife had had one miscarriage. He was a healthy man, with the exception of his nervous system, which showed typical, if rather slight, signs of locomotor ataxia. The Wassermann reaction was fairly positive. The case ran the usual course for two years. When one day at dinner, the patient, who during the day had been in his usual good general health, suddenly changed colour, and when examined was found to be dead. The second case was very similar, and the patient died suddenly while playing a game of bridge. In neither case could an autopsy be obtained, but frequent careful physical examination during life showed no evidence of arterial disease, and this could almost certainly be excluded. He considered "cardiac crisis" to be the most probable explanation of death. This condition was almost entirely overlooked in the text-books, but was mentioned by a few authors, notably by Gowers and Osler.

The President considered the record of these two cases very important; but thought that, as no cardiac symptom had previously appeared and no anginal symptoms were present during the fatal attacks, it was questionable whether they could be called cases of cardiac crisis.

Section of Obstetrics.

Friday, May 18th, 1917.

Ruptured Tubal Pregnancy.

The President of the Academy (Dr. Purffoy) showed specimens removed from two cases of ruptured tubal pregnancy.

No. I.—Fallopian tube removed from a patient, aged forty-two, fourteen years married and not previously pregnant. In the abdominal ostium a mass of decolourised clot was

adherent, surrounded by the tubal fimbriæ, and on section showing numerous chorionic villi. The abdomen was distended with several pints of blood-stained fluid.

No. II.—A young, healthy 3-para, admitted with history of irregular menstruation and slight bleeding ; moderate pelvis hæmatocele at left side. Some days previously had fallen against a chair. Condition not serious till some hours after admission. On laparotomy the tube was found to have a rent extending nearly half its length. Tubal mole found in Douglas's space.

Dilated Tube and Carneous Mole.

Sir William Smyly showed a specimen of sactosalpinx or dilated tube and a carneous mole which he had removed from a patient in the Rotunda Hospital on 8th May.

History.—K. B., married twelve years, three children, no abortions. The last pregnancy four years ago, child dead ; last menstruation March 4th. On the 12th of April a slight hæmorrhage, which continued fifteen days. On the 27th of April she was attacked with severe pain in the hypogastrium and vulvar regions and great distress during defæcation.

Examination.—On bimanual examination the uterus was found anteposed. Douglas's pouch was distended by a hæmatocele, which extended upwards and to the right above the uterus.

Operation.—Upon opening the abdomen some free blood was met with. The left tube was about the size and shape of a banana, and was removed. The right one was fairly healthy, and was left. A large quantity of clots and a carneous mole about the size of a billiard ball were removed from Douglas's pouch and the operation was completed.

On examining the specimen afterwards, no opening through which an ovum could have escaped could be found, and it was evident that the tube removed had not been the site of the pregnancy. There was a small cavity in the right ovary, which was evidently a ruptured Graafian follicle, but most probably the ovum had been extruded from the right tube, though it showed no sign to support the idea.

Section of Anatomy and Physiology.

Friday, May 25, 1917.

Perfusion of the Frog's Heart with Nicotine.

Professor Barry read a paper on the above. He said that hitherto nicotine effects on the heart were chiefly investigated by dropping on the drug diluted without neutralisation. The results of perfusion after neutralisation are much more reliable, and this was the method employed in these experiments.

Nicotine acts on nerve and on muscle, at first exciting and then depressing. The pure accelerator fibres are first stimulated, and in the fresh heart this acceleration is generally accompanied by increased tonus. Later the accelerators are depressed as shown by negative response to stimulation of the sympathetic, which prior to nicotine caused acceleration.

The inhibitor mechanism is next excited, causing a slowing of the rate and cessation of beat with .2 per cent. solution, the heart remaining quiescent during one to three or four minutes.

The purely augmentor fibres are excited early by nicotine, but this effect is best revealed after the stimulation of inhibitors has passed off. If the tone of the augmentors be increased by artificial stimulation before the drug, then the increase in force of the beat caused by this is very great. It has been shown by a simple device that the heart can perform more work in this phase, taking an augmented inflow comfortably and expelling it against increased resistance.

Atropine does not counteract nicotine slowing. Atropine is said to paralyse the vagus endings in doses of .001 mgr. or over. Amounts much greater than this were perfused for some minutes prior to nicotine perfusion, but without affecting the inhibiting action of the latter ; therefore, it is probable that nicotine excites the post-ganglionic fibres of the vagus, just as it excites post-ganglionic accelerator fibres.

A phenomenon of much interest observed frequently in the experiments was reversal of the heart beat by nicotine. The

ordinarily subordinate mechanism of the ventricle is stimulated by the drug to predominance, so that the ventricle leads the cycle. This may occur with or without cessation of the auricle beat as a preliminary, but the auricle-ventricle interval has been seen frequently to become shorter before reversal set in. It was also shown that a block could be temporarily broken down by nicotine, a 2–1 rhythm being converted to 1–1.

Its action on the muscle-connecting auricle to ventricle points rather to Engelmann's view of a dromotropic delay in the impulse from above (lowered conductivity) than to the De Boer view of a prolonged refractory phase in the ventricle.

Numerous graphic records illustrating the above points were shown.

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